Cochrane Library

Cochrane Database of Systematic Reviews

Day hospital versus admission for acute psychiatric disorders (Review)

Marshall M, Crowther R, Sledge WH, Rathbone J, Soares-Weiser K

Marshall M, Crowther R, Sledge WH, Rathbone J, Soares-Weiser K.
Day hospital versus admission for acute psychiatric disorders.
Cochrane Database of Systematic Reviews 2011, Issue 12. Art. No.: CD004026.
DOI: 10.1002/14651858.CD004026.pub2.

www.cochranelibrary.com

WILEY

TABLE OF CONTENTS

[Intervention Review]

Day hospital versus admission for acute psychiatric disorders

Max Marshall[1], Ruth Crowther[2], William Hurt Sledge[3], John Rathbone[4], Karla Soares-Weiser[5]

[1]University of Manchester, The Lantern Centre, Preston., UK. [2]School of Population Health, University of Queensland, Queensland, Australia. [3]Yale New Haven Psychiatric Hospital, Yale University, Hamden, Connecticutt, USA. [4]HEDS, ScHARR, The University of Sheffield, Sheffield, UK. [5]Enhance Reviews Ltd, Wantage, UK

Contact address: Max Marshall, University of Manchester, The Lantern Centre, Vicarage Lane, Of Watling Street Road, Fulwood, Preston., Lancashire, UK. max.marshall@manchester.ac.uk, max.marshall@lancashirecare.nhs.uk.

Editorial group: Cochrane Schizophrenia Group.
Publication status and date: New search for studies and content updated (no change to conclusions), published in Issue 12, 2011.

Citation: Marshall M, Crowther R, Sledge WH, Rathbone J, Soares-Weiser K. Day hospital versus admission for acute psychiatric disorders. *Cochrane Database of Systematic Reviews* 2011, Issue 12. Art. No.: CD004026. DOI: 10.1002/14651858.CD004026.pub2.

ABSTRACT

Background

Inpatient treatment is an expensive way of caring for people with acute psychiatric disorders. It has been proposed that many of those currently treated as inpatients could be cared for in acute psychiatric day hospitals.

Objectives

To assess the effects of day hospital versus inpatient care for people with acute psychiatric disorders.

Search methods

We searched the Cochrane Schizophrenia Group Trials Register (June 2010) which is based on regular searches of MEDLINE, EMBASE, CINAHL and PsycINFO. We approached trialists to identify unpublished studies.

Selection criteria

Randomised controlled trials of day hospital versus inpatient care, for people with acute psychiatric disorders. Studies were ineligible if a majority of participants were under 18 or over 65, or had a primary diagnosis of substance abuse or organic brain disorder.

Data collection and analysis

Two review authors independently extracted and cross-checked data. We calculated risk ratios (RR) and 95% confidence intervals (CI) for dichotomous data. We calculated weighted or standardised means for continuous data. Day hospital trials tend to present similar outcomes in slightly different formats, making it difficult to synthesise data. We therefore sought individual patient data so that we could re-analyse outcomes in a common format.

Main results

Ten trials (involving 2685 people) met the inclusion criteria. We obtained individual patient data for four trials (involving 646 people). We found no difference in the number lost to follow-up by one year between day hospital care and inpatient care (5 RCTs, n = 1694, RR 0.94 CI 0.82 to 1.08). There is moderate evidence that the duration of index admission is longer for patients in day hospital care than inpatient care (4 RCTs, n = 1582, WMD 27.47 CI 3.96 to 50.98). There is very low evidence that the duration of day patient care (adjusted days/month) is longer for patients in day hospital care than inpatient care (3 RCTs, n = 265, WMD 2.34 days/month CI 1.97 to 2.70). There is no difference between day hospital care and inpatient care for the being readmitted to in/day patient care after discharge (5 RCTs, n = 667, RR 0.91 CI 0.72 to 1.15). It is likely that there is no difference between day hospital care and inpatient care for being unemployed at the end of the study (1 RCT, n = 179, RR 0.88 CI 0.66 to 1.19), for quality of life (1 RCT, n = 1117, MD 0.01 CI -0.13 to 0.15) or for treatment satisfaction (1 RCT, n = 1117, MD 0.06 CI -0.18 to 0.30).

Authors' conclusions

Caring for people in acute day hospitals is as effective as inpatient care in treating acutely ill psychiatric patients. However, further data are still needed on the cost effectiveness of day hospitals.

PLAIN LANGUAGE SUMMARY

Day hospital versus admission for acute psychiatric disorders

Day hospitals are a less restrictive alternative to inpatient admission for people who are acutely and severely mentally ill. This review compares acute day hospital care to inpatient care. We found that at least one in five patients currently admitted to inpatient care could feasibly be cared for in an acute day hospital. Patients treated in the day hospital had the same levels of treatment satisfaction and quality of life as those cared for as inpatients. The day hospital patients were also no more likely to be unemployed at the end of their care.

SUMMARY OF FINDINGS

Summary of findings for the main comparison. Day hospital compared to Inpatient for acute psychiatric disorders

Day hospital compared to Inpatient for acute psychiatric disorders

Patient or population: patients with acute psychiatric disorders
Settings:
Intervention: day hospital
Comparison: inpatient

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No of Participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Inpatient	Day hospital				
Feasibility and engagement: lost to follow-up by 1 year Follow-up: 10 to 12 months	**Low[1]**		**RR 0.94** (0.82 to 1.08)	1694 (5 studies[2])	⊕⊕⊕⊖ **moderate** [3]	
	100 per 1000	**94 per 1000** (82 to 108)				
	Moderate[1]					
	300 per 1000	**282 per 1000** (246 to 324)				
	High[1]					
	500 per 1000	**470 per 1000** (410 to 540)				
Extent of hospital care: 1. duration of index admission Follow-up: 10 to 12 months	The mean extent of hospital care: 1. duration of index admission ranged across control groups from **-4.6 to 55.5 days**	The mean extent of hospital care: 1.		1582 (4 studies[2])	⊕⊕⊕⊖ **moderate** [3]	

		duration of index admission in the intervention groups was **27.47 higher** (3.96 to 50.98 higher)			
Extent of hospital care: 3. duration of day patient care (adjusted days/month) Follow-up: 10 to 12 months	The mean extent of hospital care: 3. duration of day patient care (adjusted days/month) ranged across control groups from **2.1 to 3.6 days /month**	The mean extent of hospital care: 3. duration of day patient care (adjusted days/month) in the intervention groups was **2.34 higher** (1.97 to 2.7 higher)		465 (3 studies)	⊕⊖⊖⊖ **very low** [4,5]
Extent of hospital care: 5. readmitted to in/day patient care after discharge Follow-up: 10 to 24 months	**Low[1]**		**RR 0.91** (0.72 to 1.15)	667 (5 studies)	⊕⊖⊖⊖ **very low** [6,7]
	100 per 1000	**91 per 1000** (72 to 115)			
	Moderate[1]				
	300 per 1000	**273 per 1000** (216 to 345)			

	High[1]				
	500 per 1000	**455 per 1000** (360 to 575)			
Unemployed (at end of study) Follow-up: 2 to 12 months	**Low[1]**		**RR 0.81** (0.67 to 0.97)	320 (2 studies)	⊕⊕⊙⊙ **low** [8,9]
	200 per 1000	**162 per 1000** (134 to 194)			
	Moderate[1]				
	600 per 1000	**486 per 1000** (402 to 582)			
	High[1]				
	900 per 1000	**729 per 1000** (603 to 873)			
Quality of life: average overall role score - at 12 months MANSA - Manchester Short Assessment of Quality of Life Follow-up: 12 months	The mean quality of life: average overall role score - at 12 months in the control groups was **0.01**	The mean quality of life: average overall role score - at 12 months in the intervention groups was **0.01 higher** (0.13 lower to 0.15 higher)		1117 (1 study[2])	⊕⊕⊕⊙ **moderate** [10]

Treatment satisfaction: average overall role score - at discharge CAT - Client Assessment of Treatment Follow-up: 12 months	The mean treatment satisfaction: average overall role score - at discharge in the control groups was **8.06 points**	The mean treatment satisfaction: average overall role score - at discharge in the intervention groups was **0.06 higher** (0.18 lower to 0.3 higher)		1117 (1 study[2])	⊕⊕⊕⊖ **moderate** [10]

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% CI) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: confidence interval; **RR:** risk ratio.

GRADE Working Group grades of evidence
High quality: further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: we are very uncertain about the estimate.

[1] Middle level of control risk approximates to that of the control risk in the trials.
[2] One large (n = 1117) high-quality multi-centre RCT (Kallert-EU-2007) provides data for all outcomes. This trial carries more weight than other pooled trials and this was taken into consideration when assessing overall risk of bias.
[3] Inconsistency: rated 'serious' - heterogeneity not explained by differences in populations/interventions. With removal of Sledge-US-1996 (high risk of bias, different results from other included trials) data become homogeneous.
[4] Risk of bias: rated 'very serious'/of 3 relevant RCTs, 1 - inadequate sequence generation and allocation concealment, none addressed incomplete data adequately. It was unclear in all whether they were free from other biases.
[5] There was heterogeneity for this outcome, which is not explained by differences in the populations and interventions used in the studies.
[6] Risk of bias: rated 'very serious'. Of 5 relevant RCTs, 2 had inadequate sequence generation, 3 had inadequate allocation concealment, none addressed incomplete data adequately and it was unclear whether any were free from other biases.
[7] Imprecision: rated 'serious'. 95% confidence intervals very wide.
[8] Risk of bias: rated 'serious'. 2 relevant RCTs, 1 had inadequate sequence generation and allocation concealment, incomplete data not addressed, it was unclear whether they were free from other biases.
[9] Publication bias: rated 'strongly suspected'. Only two studies reported on this outcome.
[10] Publication bias: rated 'strongly suspected'. Only one study reported on this outcome.

BACKGROUND

Description of the condition

Despite the growth of community care, many people with acute psychiatric disorders continue to be treated as inpatients (DoH 1996). This is an expensive way of caring for such patients (Audit Comm 1994) and surveys suggest that it is often unnecessary (Beck 1997). It has been proposed that many of those currently treated as inpatients could instead be treated in day hospitals (Pang 1985).

Description of the intervention

The psychiatric day hospital has been defined as a unit that provides "diagnostic and treatment services for acutely ill patients who would otherwise be treated on traditional psychiatric inpatient units" (Rosie 1987). The acute psychiatric day hospital is to be distinguished from other types of "partial hospitalisation" or "day care" such as transitional care for patients leaving hospital, more intensive alternatives to outpatient care (day treatment programmes) and support of long-term patients living in the community (day care centres) (Hoge 1992; Rosie 1987).

Psychiatric day hospitals were first described in the Soviet Union in the 1930s where they arose as a result of bed shortages (Volovik 1986). The first North American day hospital was opened in Montreal, Quebec in 1946, also in an attempt to reduce the demand for inpatient beds (Cameron 1947). In the USA day hospitals became a popular way of treating people in the 1960s following the 1963 Community Mental Health Center Construction Act, which set in law the need to establish partial hospitalisation programmes (Pang 1985). Similar developments encouraged the growth of day hospitals in the UK in the 1960s, and in the Netherlands and West Germany in the 1970s (Schene 1986). In the 1980s, however, research commissioned by the American Psychiatric Association showed widespread closure of partial hospitalisation programmes and a low rate of growth in the numbers of patients served by such programmes (Krizay 1989).

A number of factors appear to have contributed to the decline. First, there was a growing awareness of the limited evidence for the effectiveness and cost effectiveness of day hospitals (Creed 1989; Vaughn 1983). Second, day hospitals faced competition from more radical "non-institutional" alternatives, such as assertive community treatment (Hoge 1992). Third, confusion over the role of day hospitals led to some becoming expensive day centres, as they were overwhelmed by inappropriately placed long-term patients (Pryce 1982). Despite these problems, remorseless pressure on inpatient facilities has led to continued interest in psychiatric day hospitals and has inspired the development of new-style day hospitals augmented by outreach services, crisis beds, and extended hours programmes (Creed-UK-1996; Schene 1988; Sledge-US-1996).

How the intervention might work

Proponents have claimed that day hospitals can provide more cost-effective care by: promoting quicker recovery (Cameron 1947), improving social functioning (Greene 1981; Schene 1986), reducing family burden (Pang 1985), shortening the duration of hospital care (Parker 1990) and reducing relapse rates (Moscowitz 1980).

Why it is important to do this review

Despite 50 years of research, opinion remains divided on the cost effectiveness of day hospital treatment. Critics highlight the high rates of patients lost to follow-up in day hospital studies (Wilkinson 1984), and question whether day hospital treatment might actually 'institutionalise' patients by encouraging them to attend for overlong periods of time (Hoge 1992).

OBJECTIVES

1. Primary objective

To assess the effects of admission to a psychiatric day hospital versus admission to inpatient care for people with acute psychiatric disorders.

The main hypothesis was that admission to a day hospital would reduce the extent of hospital care and total costs of care, without any deterioration in follow-up rates or clinical and social functioning.

2. Secondary objectives

To determine:

- for what proportion of acutely ill patients day hospital treatment was feasible;
- whether patients recover at the same rate in day hospital treatment (in terms of symptoms and social functioning); and
- how far clinical and social recovery was affected by personal characteristics such as diagnosis, sex, and age.

The review was not concerned with the other modes of 'partial hospitalisation' listed above, i.e. day treatment programmes and day centres, which have been reviewed elsewhere (Marshall 2001). The use of partial hospitalisation as a form of transitional care is also reviewed elsewhere on the *Cochrane Library* (Johnstone 2001).

METHODS

Criteria for considering studies for this review

Types of studies

We considered all relevant randomised controlled trials (RCTs), as well as economic evaluations conducted alongside included RCTs. We excluded quasi-RCTs, such as those allocating by using alternate days of the week. Where trials were described in some way as to suggest or imply that the study was randomised and where the demographic details of each group's participants were similar, we included trials and undertook sensitivity analysis to the presence or absence of these data.

Types of participants

People with acute psychiatric disorders, diagnosed by any criteria, who would have been admitted to inpatient care if acute day hospital care had not been available.

Studies were not eligible if they were restricted to, or included a majority of, patients who were aged under 18 or over 65, or who had a primary diagnosis of substance abuse and/or organic brain disorder.

Types of interventions

1. Acute psychiatric day hospitals

We have defined these as units that provided diagnostic and treatment services for acutely ill patients who would otherwise be treated on traditional psychiatric inpatient units.

2. Standard inpatient care

Types of outcome measures

We analysed the following outcomes for different lengths of follow-up: up to three months, six months or more than six months.

Primary outcomes

1. Lost to follow-up

Secondary outcomes

1. Feasibility and engagement

1.1 Unsuitable for day patient care

2. Extent of hospital care

2.1 Duration of initial admission
2.2 Days in inpatient care
2.3 Days in day patient care
2.4 Days in inpatient or day patient care
2.5 Re-admitted to inpatient or day patient care after discharge

3. Clinical and social outcomes

3.1 Mental state
3.2 Social functioning
3.3 Burden on carers
3.4 Deaths
3.5 Employed at end of study
3.6 Satisfaction with care
3.7 Quality of life

4. Costs of care

4.1 Cost of index admission
4.2 Cost of hospital care (mean monthly - comprising cost of index admission plus cost of subsequent admissions)
4.3 Cost of psychiatric care (mean monthly - comprising cost of hospital care plus cost of all ambulatory psychiatric care)
4.4 Cost of all care (mean monthly - comprising cost of psychiatric care plus costs of other medical/social care, but excluding wages, costs to relatives, and transfer payments)

Search methods for identification of studies

Electronic searches

1. Cochrane Schizophrenia Group Trials Register (June 2010)

We searched the register using the phrase:

(day?care* or day?cent* or day?hosp* in interventions field in STUDY)]

This register is compiled by systematic searches of major databases, hand searches and conference proceedings (see group module).

For details of previous electronic search - please see Appendix 1.

Searching other resources

1. Reference searching

We inspected references of all identified studies for further relevant studies.

2. Personal contact

We contacted the first author of each included study for information regarding unpublished trials.

Data collection and analysis

Selection of studies

In the first version of this review, MM and AA independently inspected abstracts of the reports identified by the search. We identified potentially relevant abstracts (i.e. those in which a group of day hospital patients meeting the patient inclusion criteria were compared against a control group) and ordered full papers. A reliability study found complete agreement on which trials met inclusion criteria.

In the latest version, reviewer NM inspected all abstracts of studies identified as above and identified potentially relevant reports. In addition, to ensure reliability, KSW inspected a random sample of these abstracts, comprising 10% of the total. Where disagreement occurred we resolved this by discussion, or where there was still doubt, we acquired the full article for further inspection. When we had acquired the full articles of relevant reports for reassessment, we carefully inspected for a final decision on inclusion (see Criteria for considering studies for this review). Once we had obtained the full articles, NM and KSW in turn inspected all full reports and independently decided whether they met inclusion criteria. NM and KSW were not blinded to the names of the authors, institutions or journal of publication. Where difficulties or disputes arose, we asked author JM for help and if it was impossible to decide, added these studies to those awaiting assessment and contacted the authors of the papers for clarification.

Data extraction and management

1. Extraction

1.1 Data regarding criteria and outcomes

In the first version of the review, where further clarification was needed, we contacted the authors of trials to provide missing data. We sought individual patient data for all patients randomised in eligible trials (published or unpublished). We verified all individual patient data received against the original trial reports. We resolved any queries by contacting the trialists. For trials where individual patient data were not available, two authors extracted categorical and continuous data separately from trial reports and another cross checked (MM and either AA or RC).

In the latest version, authors NM and KSW independently extracted data from the single included study. We discussed any disagreement and documented decisions. With remaining problems JM helped clarify issues and we documented those final decisions. We extracted data presented only in graphs and figures whenever possible, but included them only if two authors independently had the same result.

1.2 Additional data

1.2.1 Feasibility of hospital treatment

We have defined the feasibility of day hospital treatment as the percentage reduction in acute inpatient admissions that could be achieved by diverting patients to an acute day hospital. We estimated feasibility by a modification of the method suggested by Kluiter (Wiersma-NL-1989), the general formula being: 100 x number engaging in day hospital treatment/(number assessed for eligibility x R), where R is the randomisation ratio for the trial (defined as number randomised to day hospital divided by number of patients randomised). However, estimates of feasibility are profoundly affected by judgements about what is 'engagement' in day hospital treatment and how many patients have been assessed for eligibility. We therefore decided to perform a sensitivity analysis to give a best and worst estimate of feasibility for each included trial.

We based the best estimate on defining: i. engagement in day hospital as the number randomised to day hospital treatment; and ii. assessed for eligibility as the number remaining after exclusions for administrative reasons. We defined patients excluded for administrative reasons as those who were too well to be randomised to day care, left before they could be assessed or lived outside the study catchment area. We based the worst estimate of feasibility on defining: i. engagement in day hospital as the number randomised to day hospital treatment (those admitted as inpatients in the first four weeks + the number of day patients who did not turn up for day hospital treatment); and ii. assessed for eligibility as the number presenting for admission before any administrative exclusions were made. We derived a weighted average for the best and worst estimates of feasibility derived in this way. However, for a minority of trials (referred to as 'Type 2' trials, see Description of studies below), we could not apply this formula for calculating feasibility because all patients were admitted to inpatient care before randomisation to continuing inpatient care or day hospital care. For these trials, we calculated a single estimate of feasibility, based on those patients randomised to day hospital care who experienced only a brief episode of inpatient care before transfer to a day hospital. We estimated number lost to follow-up by taking the number who were not re-interviewed at the final follow-up assessment. We assumed that clients lost to follow-up also dropped out of care.

1.2.2 Economic data

We have not combined individual patient data on economic variables across trials because there is no agreed method for overcoming the problems caused by differences in costing methodology between trials and between countries. Instead, we have presented these data adjusted to a common format (see Types of outcome measures above) in the currencies used in the original trials. We then calculated percentage differences in costs between treatment and control conditions and, where possible, compared costs of treatment and control care using non-parametric tests. For Creed-UK-1990, we calculated costs of hospital care using individual patient data, working on the assumption that the relative costs of day hospital and inpatient care were similar to those reported in Creed-UK-1996 (both trials took place in the same day hospital with the same general hospital control).

2. Management

2.1 Forms

We extracted data onto standard, simple forms.

2.2 Scale-derived data

We included continuous data from rating scales only if:

a) the psychometric properties of the measuring instrument had been described in a peer-reviewed journal (Marshall 2000); and
b) the measuring instrument was not written or modified by one of the trialists for that particular trial; and
c) the measuring instrument is either i. a self-report or ii. completed by an independent rater or relative (not the therapist).

2.3 Endpoint versus change data

We preferred to use scale endpoint data, which typically cannot have negative values and are easier to interpret from a clinical point of view. Change data are often not ordinal and are very problematic to interpret. If endpoint data were unavailable, we used change data.

2.4 Skewed data

2.4.1 General

Continuous data on clinical and social outcomes are often not normally distributed. To avoid the pitfall of applying parametric tests to non-parametric data, we aim to apply the following standards to all data before inclusion: a) standard deviations and means are reported in the paper or obtainable from the authors; b) when a scale starts from the finite number zero, the standard deviation, when multiplied by two, is less than the mean (as otherwise the mean is unlikely to be an appropriate measure of the centre of the distribution, (Altman 1996); c) if a scale starts from a positive value (such as PANSS which can have values from 30 to 210) we will modify the calculation described above to take the scale starting point into account. In these cases skew is present if 2SD>(S-S min), where S is the mean score and S min is the minimum score. Endpoint scores on scales often have a finite start and end point and these rules can be applied. When continuous data are presented on a scale which includes a possibility of negative values (such as change data), it is difficult to tell whether data are skewed or not. We entered skewed data from studies of less than 200 participants in additional tables rather than into an analysis. Skewed data pose less of a problem when looking at means if the sample size is large and were entered into syntheses.

2.4.2 Specific

Data concerning use of hospital care were skewed, but we have nonetheless presented them on Review Manager (RevMan 2008) to facilitate comparison between trials. However, the results of any parametric analyses on these data were cross-checked using the non-parametric Mann-Whitney U statistic.

2.5 Common measure

To facilitate comparison between trials, we converted variables that can be reported in different metrics, such as days in hospital (mean days per year, per week or per month) to a common metric (e.g. mean days per month). We adjusted time spent in the day hospital so that 'days in day hospital' represented the actual number of attendances at the day hospital (excluding missed days), rather than the total time for which the patient was a day hospital patient

(except in the case of duration of initial admission). Creed-UK-1990 did not distinguish between duration of care and actual number of attendances, so actual number of attendances was estimated using the same ratio of duration: actual attendances reported in Creed-UK-1996 (which took place in the same day hospital using the same hospital control).

2.6 Conversion of continuous to binary

Where possible, we attempted to convert outcome measures to dichotomous data. This could be done by identifying cut-off points on rating scales and dividing participants accordingly into 'clinically improved' or 'not clinically improved'. We generally assumed that, if there had been a 50% reduction in a scale-derived score such as the Brief Psychiatric Rating Scale (BPRS, Overall 1962) or the Positive and Negative Syndrome Scale (PANSS, Kay 1986), this could be considered as a clinically significant response (Leucht 2005a; Leucht 2005b). If data based on these thresholds were not available, we used the primary cut-off presented by the original authors.

2.7 Direction of graphs

Where possible, we entered data in such a way that the area to the left of the line of no effect indicates a favourable outcome for acute day hospital care.

2.8 Summary of findings table

We included the following short- or medium-term outcomes in a summary of findings table. (KSW was not biased by being familiar with the data.)

1. Discontinuation of treatment

2. Extent of hospital care

- Duration of index admission
- Days in day patient care
- Readmitted to in/day patient care after discharge

3. Clinical and social outcomes

- Unemployed
- Quality of life
- Treatment satisfaction

4. Costs of care

- Cost of all care (mean monthly - comprising cost of psychiatric care plus costs of other medical/social care, but excluding wages, costs to relatives, and transfer payments).

Assessment of risk of bias in included studies

KSW and NM independently assessed the risk of bias of each trial using The Cochrane Collaboration's risk of bias tool (Higgins 2009). We created a form following the guidance to make judgments on the risk of bias in six domains: sequence generation; allocation concealment; blinding (of participants, personnel, and outcome assessors); incomplete outcome data; selective outcome reporting; and other sources of bias. We categorised these judgments as 'yes' (low risk of bias), 'no' (high risk of bias), or 'unclear'. We resolved disagreements through discussion and by consulting MM.

Measures of treatment effect

1. Binary data

For binary outcomes we calculated a standard estimation of the risk ratio (RR) and its 95% confidence interval (CI). It has been shown that RR is more intuitive (Boissel 1999) than odds ratios and that odds ratios tend to be interpreted as RR by clinicians (Deeks 2000). For statistically significant results we had planned to calculate the number needed to treat to provide benefit/to induce harm statistic (NNTB/H), and its 95% CI using Visual Rx (http://www.nntonline.net/), taking account of the event rate in the control group. This, however, was superseded by Summary of findings for the main comparison and the calculations therein.

2. Continuous data

For continuous outcomes we estimated a random-effects mean difference (MD) between groups. We preferred not to calculate effect size measures (standardised mean difference (SMD)). However, in the case of where scales were of such similarity to allow presuming there was a small difference in measurement, we calculated it and, whenever possible, we transformed the effect back to the units of one or more of the specific instruments.

Unit of analysis issues

1. Cluster trials

Studies increasingly employ 'cluster randomisation' (such as randomisation by clinician or practice) but analysis and pooling of clustered data poses problems. Authors often fail to account for intraclass correlation in clustered studies, leading to a 'unit of analysis' error (Divine 1992) whereby P values are spuriously low, CI unduly narrow and statistical significance overestimated. This causes type I errors (Bland 1997; Gulliford 1999).

Where clustering is not accounted for in primary studies, we presented data in a table, with a (*) symbol to indicate the presence of a probable unit of analysis error. In subsequent versions of this review we will seek to contact first authors of studies to obtain intraclass correlation coefficients for their clustered data and to adjust for this by using accepted methods (Gulliford 1999). Where clustering had been incorporated into the analysis of primary studies, we present these data as if from a non-cluster randomised study, but adjusted for the clustering effect.

We have sought statistical advice and have been advised that the binary data as presented in a report should be divided by a 'design effect'. This is calculated using the mean number of participants per cluster (m) and the intraclass correlation coefficient (ICC) [Design effect =1+(m-1)*ICC] (Donner 2002). If the ICC was not reported it was assumed to be 0.1 (Ukoumunne 1999).

If cluster studies had been appropriately analysed, taking into account intraclass correlation coefficients and relevant data documented in the report, synthesis with other studies would have been possible using the generic inverse variance technique.

2. Cross-over trials

A major concern of cross-over trials is the carry-over effect. It occurs if an effect (e.g. pharmacological, physiological or psychological) of the treatment in the first phase is carried over to the second phase. As a consequence, on entry to the second phase the participants can differ systematically from their initial state despite a wash-out

phase. For the same reason cross-over trials are not appropriate if the condition of interest is unstable (Elbourne 2002). As both effects are very likely in severe mental illness, we will only use data of the first phase of cross-over studies.

3. Studies with multiple treatment groups

Where a study involved more than two treatment arms, if relevant, we have presented the additional treatment arms in comparisons. Where the additional treatment arms were not relevant, we have not reproduced these data.

Dealing with missing data

1. Overall loss of credibility

At some degree of loss of follow-up data must lose credibility (Xia 2007). For any particular outcome, should more than 50% of data be unaccounted for, we did not reproduce these data or use them within analyses. If, however, more than 50% of those in one arm of a study were lost, but the total loss was less than 50%, we marked such data with (*) to indicate that such a result may well be prone to bias.

2. Binary

In the case where attrition for a binary outcome is between 0% and 50% and where these data were not clearly described, we have presented data on a 'once-randomised-always-analyse' basis (an intention-to-treat analysis). We assumed that those leaving the study early had the same rates of negative outcome as those who completed, with the exception of the outcome of death. We undertook a sensitivity analysis testing how prone the primary outcomes were to change when 'completed' data only were compared to the intention-to-treat analysis using the above assumption.

3. Continuous

3.1 Attrition

In the case where attrition for a continuous outcome is between 0% and 50% and completer-only data were reported, we have reproduced these.

3.2 Standard deviations

We first tried to obtain the missing values from the authors. If not available, where there were missing measures of variance for continuous data but an exact standard error and confidence interval were available for group means, and either P value or T value were available for differences in mean, we calculated them according to the rules described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2009). When only the standard error (SE) is reported, standard deviations (SDs) are calculated by the formula SD =SE * square root (n). Chapters 7.7.3 and 16.1.3 of the *Handbook* (Higgins 2009) present detailed formula for estimating SDs from P values, T or F values, confidence intervals, ranges or other statistics. If these formulae do not apply, we calculated the SDs according to a validated imputation method which is based on the SDs of the other included studies (Furukawa 2006). Although some of these imputation strategies can introduce error, the alternative would be to exclude a given study's outcome and thus to lose information. We nevertheless examined the validity of the imputations in a sensitivity analysis excluding imputed values.

3.3 Last observation carried forward

We anticipated that in some studies the method of last observation carried forward (LOCF) would be employed within the study report. As with all methods of imputation to deal with missing data, LOCF introduces uncertainty about the reliability of the results. Therefore, where LOCF data have been used in the trial, if less than 50% of the data have been assumed, we reproduced these data and indicated that they are the product of LOCF assumptions.

Assessment of heterogeneity

1. Clinical heterogeneity

We considered all included studies initially, without seeing comparison data, to judge clinical heterogeneity. We simply inspected all studies for clearly outlying situations or people which we had not predicted would arise. When such situations or participant groups arose, we fully discussed these.

2. Methodological heterogeneity

We considered all included studies initially, without seeing comparison data, to judge methodological heterogeneity. We simply inspected all studies for clearly outlying methods which we had not predicted would arise. Should such methodological outliers arise, we will fully discuss these.

3. Statistical heterogeneity

3.1 Visual inspection

We visually inspected graphs to investigate the possibility of statistical heterogeneity.

3.2 Employing the I^2 statistic

We investigated heterogeneity between studies by considering the I^2 method alongside the Chi^2 P value. The I^2 provides an estimate of the percentage of inconsistency thought to be due to chance (Higgins 2003). The importance of the observed value of I^2 depends on i. magnitude and direction of effects and ii. strength of evidence for heterogeneity (e.g. P value from Chi^2 test, or a CI for I^2). We interpreted I^2 estimate greater than or equal to 50% accompanied by a statistically significant Chi^2 statistic, as evidence of substantial levels of heterogeneity (Section 9.5.2 - Higgins 2009). When we found substantial levels of heterogeneity in the primary outcome, we explored reasons for heterogeneity (Subgroup analysis and investigation of heterogeneity).

Assessment of reporting biases

Reporting biases arise when the dissemination of research findings is influenced by the nature and direction of results (Egger 1997). These are described in Section 10 of the *Handbook* (Higgins 2009). We are aware that funnel plots may be useful in investigating reporting biases but are of limited power to detect small-study effects. We did not use funnel plots for outcomes where there were 10 or fewer studies, or where all studies were of similar sizes. In other cases, where funnel plots were possible, we sought statistical advice in their interpretation.

Data synthesis

Where possible we employed a random-effects model for analyses. We understand that there is no closed argument for preference for use of fixed-effect or random-effects models. The random-

effects method incorporates an assumption that different studies are estimating different, yet related, intervention effects. According to our hypothesis of an existing variation across studies, to be explored further in the meta-regression analysis despite being cautious that random-effects methods does put added weight onto the smaller of the studies - we favoured using random-effects model.

Subgroup analysis and investigation of heterogeneity

1. Subgroup analyses

We did not plan a subgroup analysis. However, we did undertake one for discontinuation of treatment due to satisfaction with care, adverse events or costs of care.

2. Investigation of heterogeneity

If inconsistency was high, we have reported this. First we investigated whether data had been entered correctly. Second, if data had been correct, we visually inspected the graph and successively removed studies outside of the company of the rest to see if heterogeneity was restored. Should this occur with no more than 10% of the data being excluded, we have presented data. If not, we have not pooled data and have discussed relevant issues.

Should unanticipated clinical or methodological heterogeneity be obvious, we simply stated hypotheses regarding these for future reviews or versions of this review. We did not anticipate undertaking analyses relating to these.

Sensitivity analysis

1. Implication of randomisation

We aimed to include trials in a sensitivity analysis if they were described in some way as to imply randomisation. For the primary outcomes we included these studies and if there was no substantive difference when the implied randomised studies were added to those with better description of randomisation, then we have employed all data from these studies.

2. Assumptions for lost binary data

Where assumptions had to be made regarding people lost to follow-up (Dealing with missing data), we compared the findings of the primary outcomes when we used our assumption compared with completer data only. If there was a substantial difference, we reported results and discuss them but continue to employ our assumption.

Where assumptions had to be made regarding missing SDs data (Dealing with missing data), we compared the findings on primary outcomes when we used our assumption compared with complete data only. We undertook a sensitivity analysis testing how prone results were to change when 'complete' data only were compared to the imputed data using the above assumption. If there was a substantial difference, we have reported results and discussed them, but continue to employ our assumption.

3. Published and unpublished data

We included both published and unpublished data and separated them in the sensitivity analysis. If there was no substantive difference when the unpublished data were added to the data from published trials, then we employed all data from these studies.

RESULTS

Description of studies

Please see Characteristics of included studies, Characteristics of excluded studies, and Characteristics of studies awaiting classification.

Results of the search

The 2010 update search identified 162 references (from 124 studies). Agreement about which reports may have been randomised was total and we selected and ordered 55 of the original reports. One of these reports is a new study to this review (Kallert-EU-2007) and two have been added to those awaiting assessment (Donnison 2001; Gjonbalaj-Marovic 2005). Four reports were additional references to already included studies (Figure 1).

Figure 1. Study flow diagram - 2010 update

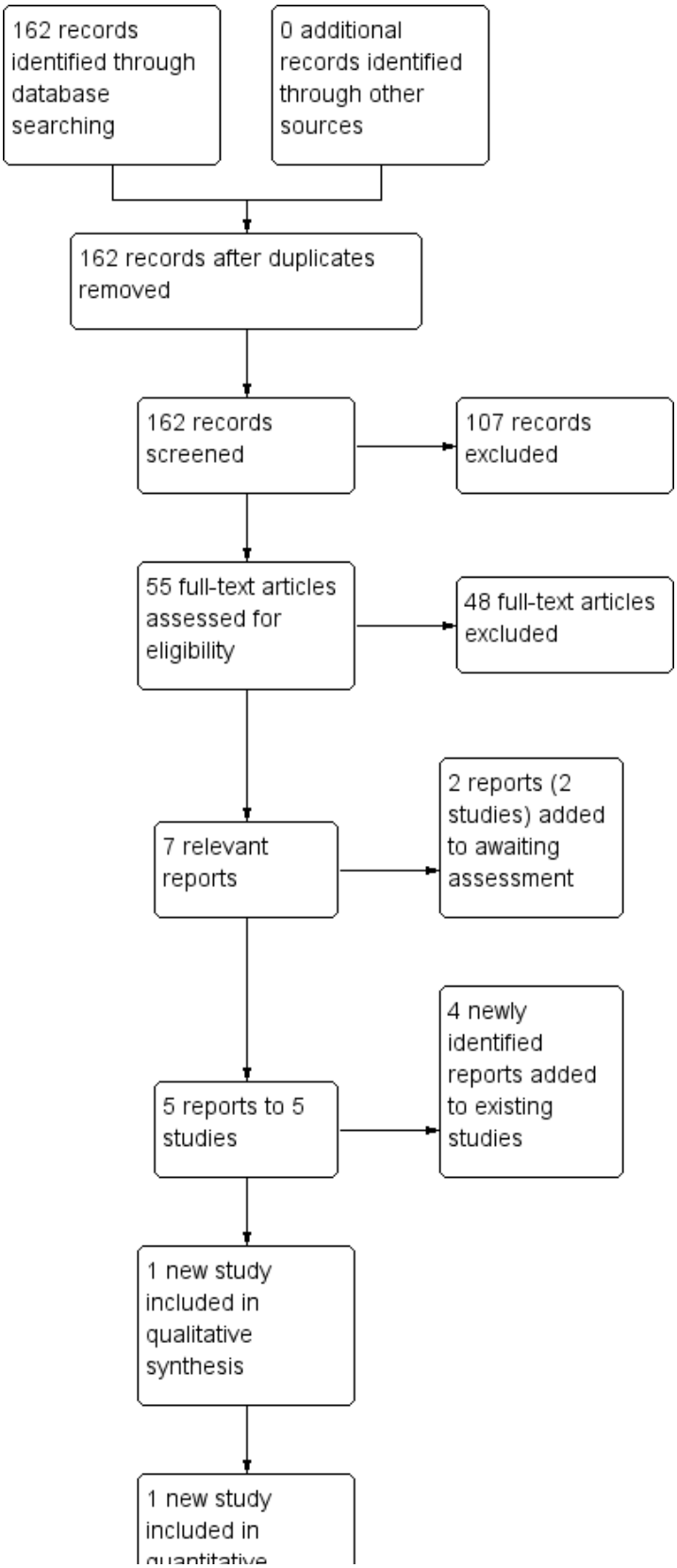
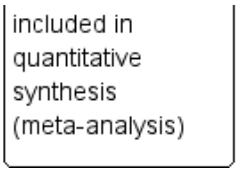


Figure 1. (Continued)



Included studies

The current review includes 46 reports describing 10 studies (Creed-UK-1990; Creed-UK-1996; Dick-UK-1985; Herz-US-1971; Kallert-EU-2007; Kris-US-1965; Schene-NL-1993; Sledge-US-1996; Wiersma-NL-1989; Zwerling-US-1964). This review now includes data on 2685 randomised people from within these 10 separate trials.

1. Methods

All studies were stated to be randomised. Sledge-US-1996, however, once people were randomised, would give the other treatment package if the treatment of allocation was not available. None of the 10 trials used evaluators who were blind to group allocation, but eight used people to rate outcome who were independent of the trialists and carers. In Kris-US-1965 and Schene-NL-1993, it was unclear if the evaluators were independent. For further details please see Risk of bias in included studies (sections on Allocation and Blinding).

2. Design

2.1 Pre-randomisation exclusions vs everyone randomised

We found included trials to be of two types. Type 1 trials excluded, before randomisation, any who were considered ineligible for day hospital treatment (for example, people who were too violent or under compulsion). The Type 1 trials were Creed-UK-1990, Creed-UK-1996, Dick-UK-1985, Herz-US-1971, Kallert-EU-2007, Kris-US-1965, Schene-NL-1993, Sledge-US-1996. Type 2 trials randomised everyone presenting for admission regardless of suitability, but admitted to the inpatient ward any people allocated to day hospital who were too unwell for immediate day hospital treatment. The Type 2 trials were Wiersma-NL-1989 and Zwerling-US-1964. The methodological differences between Type 1 and Type 2 trials meant that it would not have been sensible to analyse in the same comparison.

3. Duration

The follow-up periods of the trials were: 2 months (Kris-US-1965); 6 months (Schene-NL-1993); 10 months (Sledge-US-1996); 12 months (Creed-UK-1990; Creed-UK-1996; Dick-UK-1985; Kallert-EU-2007); and 24 months (Herz-US-1971; Wiersma-NL-1989; Zwerling-US-1964). In two trials (Kallert-EU-2007; Sledge-US-1996) the follow-up period began on discharge from inpatient/day patient care, whereas in the others it began on the day of randomisation.

4. Participants

Participants now total 2685 people. These were both men and women, mostly aged between 30 and 50 years of age, with diagnoses of various acute psychiatric disorders, but mainly schizophrenia and mood disorders. Only Kallert-EU-2007 reported a pre-trial power calculation. The trials in descending order of size were: Kallert-EU-2007 (1117); Zwerling-US-1964 (378); Schene-NL-1993 (222); Sledge-US-1996 (197); Creed-UK-1996 (187); Wiersma-NL-1989 (160); Kris-US-1965 (141); Creed-UK-1990 (102); Dick-UK-1985 (91) and Herz-US-1971 (90).

5. Setting

All trials except Wiersma-NL-1989 recruited from a population who would otherwise have been admitted to a general adult psychiatric ward. Two trials took place in the same day hospital in an inner city area of Manchester, UK (Creed-UK-1990; Creed-UK-1996). In the earlier trial, eligible patients were voluntary patients who were not too ill for day care, and who had no social factors that made day care impractical (such as being of no fixed abode). In addition to these criteria, the later trial excluded patients with organic brain disease or mania. Dick-UK-1985 took place in an acute day hospital in Dundee, Scotland. Patients were excluded if day hospital treatment was judged impractical or they were considered too ill or suicidal. Herz-US-1971 took place in an acute day hospital in New York State, USA. Patients were excluded if day care was judged impractical or if they were considered too ill or too well for day care. Kallert-EU-2007 was a multi-centre study with five sites: Dresden, Germany; London, UK; Wroclaw, Poland; Michalovce, Slovak Republic; and Prague, Czech Republic. Patients were included if they were in need of acute admission to a psychiatric facility and excluded if it was an involuntary admission, they lived too far from the hospital or were homeless, acute intoxication, addictive disorder, or required inpatient care. Kris-US-1965 took place in an acute day hospital in New York, USA. Patients were eligible if they had had a previous admission for a psychotic disorder. Schene-NL-1993 took place in an acute day hospital at the University of Utrecht, Netherlands. Patients were excluded if there were contraindications to day hospital treatment (not specified) or they had organic brain disease or a primary diagnosis of substance abuse or mental retardation. Sledge-US-1996 took place at a community mental health centre day hospital in New Haven, Connecticut, USA. The day hospital was closely linked to a crisis residence run by a non-profit organisation. Patients were excluded if they were; involuntary, not living locally, too ill for day patient treatment, intoxicated, or physically unwell. Wiersma-NL-1989 took place in a day hospital operated by the Regional Institute for Ambulatory Mental Health Care in Groningen, Netherlands. All patients presenting for inpatient care were included in the trial except for forensic patients on court orders and patients with dementia. No prior assessment was made of suitability for day hospital treatment. Patients randomised to day hospital treatment who were too unwell for immediate transfer were treated as inpatients but transferred to day hospital care as soon as feasible. Zwerling-US-1964 took place in a day hospital in New York, USA.

6. Interventions

In Creed-UK-1990, eight nurses and three occupational therapists staffed the day hospital with input from three consultant

psychiatrists. In Creed-UK-1996, the day hospital had similar staffing levels to Creed-UK-1990, but there was additional input from a community psychiatric nurse (who could visit patients who failed to turn up for treatment) and an out of hours on-call service for day patients. In Dick-UK-1985 the day hospital was staffed by two trained staff and an occupational therapist and had a staff-patient ratio of 1:12.5. The day hospital offered individual counselling, groups, activities and medication. In Herz-US-1971 the day hospital offered group-oriented psychotherapy; staffing levels were not reported. In Kallert-EU-2007 the day hospitals provided between 15 and 35 places, with mean staff hours per week per treatment place ranging from 8.8 to 16.0. General clinical expertise was high in all centres. Within the centres, the day hospital and inpatient settings varied, but not systematically. In the Dresden day hospital they specialised in outreach activities and vocational rehabilitation, and in Wroclaw there were similar differences; in London “psychological interventions” for inpatients were limited to supportive talks; in Wroclaw and Michalovce there was a low level of general hospitals. In Prague, the there were no differences between the settings. In Kris-US-1965, the day hospital offered milieu and group therapy; staffing levels were not reported. In Schene-NL-1993, the day hospital offered psychosocial therapy and had a staff:patient ratio of 1:12.5. In Sledge-US-1996, the day hospital was a 20-patient facility staffed by doctors, nurses, social workers and other therapists. Treatment emphasised group work, control of symptoms and improvement in daily living skills. The day hospital was linked to a crisis residence, which was a three-bedroom apartment supported by a crisis respite unit. In Wiersma-NL-1989, the day hospital was supported by integrated ambulatory and domiciliary care and by a back-up bed on the inpatient ward. A 24-hour telephone help-line was available to all day hospital patients. The day hospital offered a multi-disciplinary treatment programme, but staffing levels were not reported. In Zwerling-US-1964, the day hospital offered group-oriented activities and family therapy for up to 30 patients. Staffing consisted of four full-time nurses, four nurse's aides, a clinical psychologist, a social worker and dedicated time from senior and junior psychiatrists.

7. Outcomes

7.1 Intention-to-treat analysis

Schene-NL-1993 and Zwerling-US-1964 were not carried out on an intention-to-treat basis (see Risk of bias in included studies below) and so reported data on feasibility only. We did not seek individual patient data for these trials as they could not be analysed on an intention-to-treat basis. Kallert-EU-2007 was intention-to-treat, although we did not seek individual patient data for this trial.

7.2 Individual patient data

We sought these for seven other trials and obtained them for four (Creed-UK-1990; Creed-UK-1996; Sledge-US-1996; Wiersma-NL-1989). These individual patient data covered 646 patients. Of the three remaining trials, contact with the trialists confirmed that individual patient data were no longer available for Dick-UK-1985 or Herz-US-1971. We were unable to locate the trialists for Kris-US-1965.

7.3 Missing outcomes

After taking individual patient data into account, trials provided useable data on all the outcomes defined under 'Types of outcome measures' above.

7.4 Continuous outcomes

We have provided details of the scales that supplied useable data for this review below. We have provided reasons for exclusion of data from other scales in the 'Outcomes' column of the Characteristics of included studies tables.

a. Mental state

i. Present State Examination (Wing 1972)
This was used in Creed-UK-1990 and Wiersma-NL-1989. This is a clinician-rated scale measuring mental status. One hundred and forty symptom items are rated and combined to give various syndrome and sub-syndrome scores. Higher scores indicate increased severity of psychiatric symptoms.

ii. Comprehensive Psychopathology Rating Scale (Asberg 1978)
This was used in Creed-UK-1996. A four-point scale is used to rate 40 items, and 25 items are rated by observation using the same scale. Global rating of the illness is an additional item. Higher scores indicate increased severity of psychiatric symptoms.

iii. Brief Psychopathology Rating Scale (BPRS, Overall 1962)
This was used in Kallert-EU-2007 and Sledge-US-1996. A brief rating scale used to assess the severity of a range of psychiatric symptoms, including psychotic symptoms. The scale has 16 items, and each item can be defined on a seven-point scale varying from 'not present' (0) to 'extremely severe' (6).

iv. Clinical Interview Schedule (Goldberg 1972)
This was used in Dick-UK-1985. Scoring method is unclear in this particular trial, "twice the sum of the mental state ratings was added to the sum of the symptom ratings to give an overall severity score". Higher scores indicate increased severity of psychiatric symptoms.

b. Social functioning

i. Social Behaviour Assessment Schedule (Platt 1981)
This was used in Creed-UK-1990 & Creed-UK-1996. This scale yields scores in three areas: social role performance (used here), abnormal behaviours (not used) and burden on relatives (used below). Higher scores indicate greater social dysfunction.

ii. Social Adjustment Schedule (SAS, Weissman 1981)
This was used in Sledge-US-1996. Measures social functioning in a number of life domains (work, social, extended family, marital, parental, family unit and economic adequacy) on a scale of 1-7. Lower scores indicate poorer functioning.

iii. Groningen Social Disabilities Schedule (Wiersma 1988)
This was used in Wiersma-NL-1989. Rated on a scale of 0 to 4, with higher scores indicating greater social disability.

iv. Groningen Social Disabilities Schedule, Second Revision (GSDS II, Wiersma 1990)
This was used in Kallert-EU-2007. Rating are assigned for nine different social roles and for each dimension of the role. The sum score is based on overall role ratings, from 0 ('no disability') to 3 ('severe disability').

c. Burden on relatives

i. Social Behaviour Assessment Schedule (burden sub-scale, Platt 1981)

This was used in Creed-UK-1990. This is a large structured interview-based (329 questions) instrument to assess disturbed behaviour, social performance and burden on household/home/institute personnel. Extensive training is needed and the administration of the SBAS takes approximately one hour. The burden section has been used on its own and the 35 items are always applicable to all participants; it is the score of these items that is often used for comparative studies. All items are to be scored 0-3 (no distress, distress, resignation). The time window is at least one month. The SBAS score is higher in lower-class families and increases with duration of illness.

d. Treatment satisfaction

i. Client Assessment of Treatment (CAT, Priebe 1995)
This was used in Kallert-EU-2007. This questionnaire comprises seven 11-point visual analogue rating scales, which ranged from ('not at all satisfied') to 10 ('yes, entirely satisfied').

e. Quality of life

i. Manchester Short Assessment of Quality of Life (MANSA, Oliver 1996)
This was used in Kallert-EU-2007. This is a modified version of the Lancashire Quality of Life Profile consisting of subjective ratings of satisfaction with life as a whole and with specific life domains. The rating scale on each item ranged from 1 ('could not be worse') to 7 ('could not be better').

Excluded studies

In the first version of this review we excluded 64 studies. In the latest version, we have excluded a further three studies from the review. One was not randomised (Dal Santo 2004), one was a systematic review (Shek 2009) and one did not test day hospital care as the intervention (Davidson 2006).

8. Awaiting classification

One trial, in the German language, is awaiting translation (Vietze-Germany).

Risk of bias in included studies

We prepared a risk of bias assessment for each trial. For multi-centre trials providing data for single centres, we did not assess the risk of bias for each centre. Our judgments regarding the overall risk of bias in individual studies is illustrated in Figure 2 and Figure 3.

Figure 2. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.

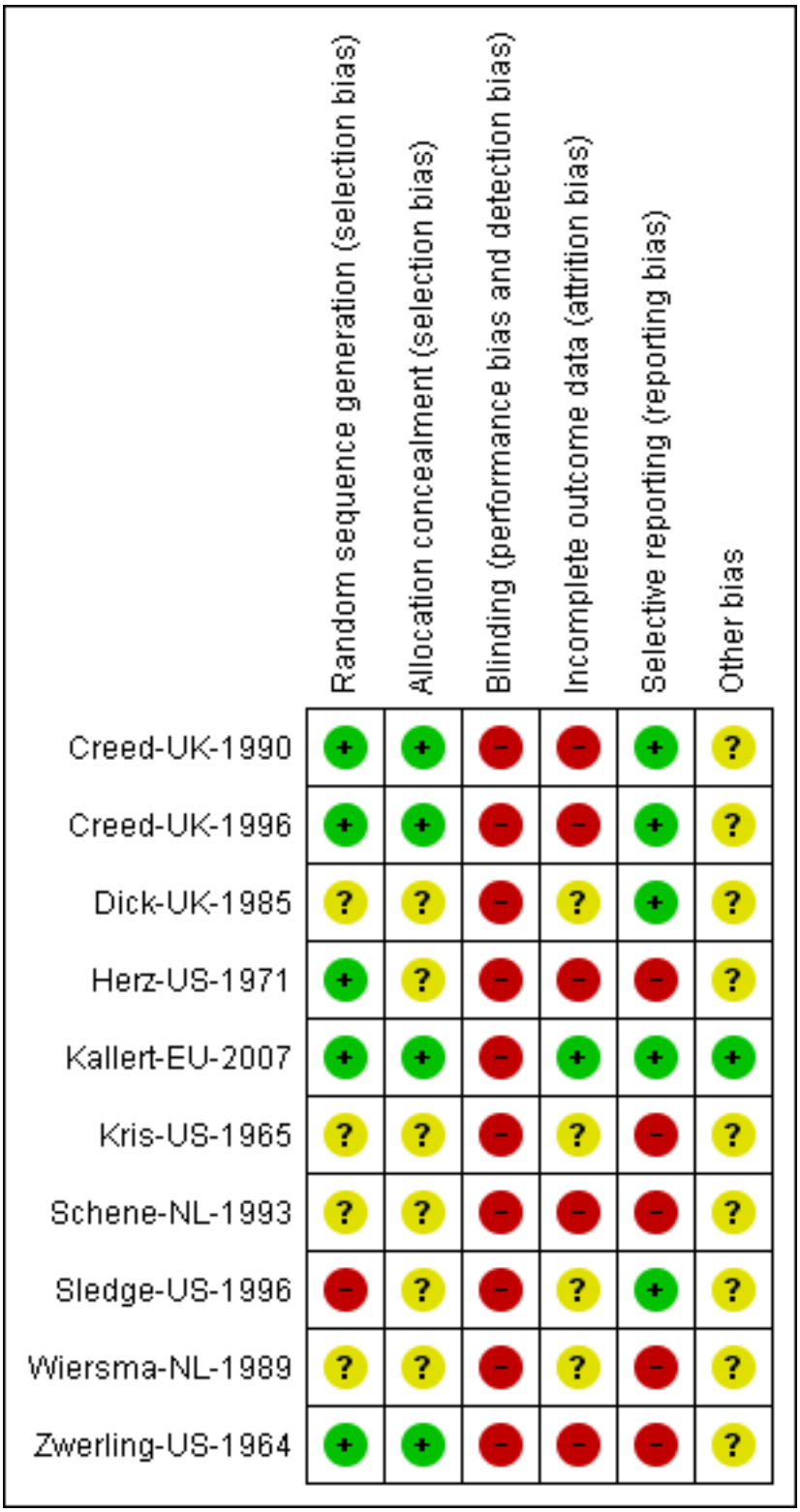
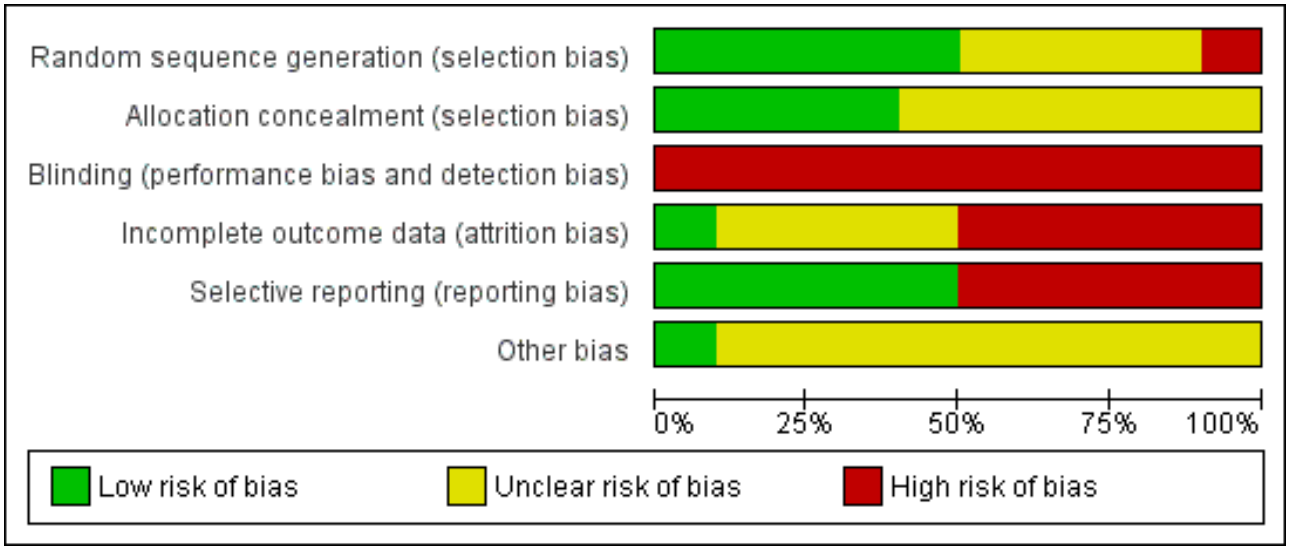


Figure 3. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies.



Allocation

Of the 10 trials analysed in this review, five reported an adequate generation of allocation sequence, one trial did not have an adequate sequence generation (Sledge-US-1996) and the method of assignment was unclear in the remaining studies. Similarly, the methods used to conceal allocation were reported as adequate in four trials and unclear in the remaining studies.

Blinding

Blinding of participants, care providers, or outcome assessors was not possible in any of the trials due to the nature of the interventions.

Incomplete outcome data

Incomplete data was addressed in one of the 10 studies, was unclear in four studies, and was not addressed adequately in the remaining trials.

Selective reporting

Five studies were free from selective reporting. In all the trials except Kallert-EU-2007, it was unclear whether they were free from other biases.

Other potential sources of bias

1. Individual patient data

No substantial discrepancies were noted between the summary data in published reports and the summary data calculated from individual patient data, thus indicating that the correct data sets had been obtained.

2. Changes in the nature of day hospital treatment

It was noted that in four of the more recent trials, day hospital care was augmented by sleep-over facilities (Sledge-US-1996) or outreach services (Creed-UK-1996; Kallert-EU-2007; Wiersma-NL-1989). This suggests that day hospital practice may be evolving over time and so it is recommended that trials are viewed sorted by year in analyses.

Effects of interventions

See: **Summary of findings for the main comparison** Day hospital compared to Inpatient for acute psychiatric disorders

For methodological reasons it was necessary to carry out separate comparisons for Type 1 and Type 2 trials (see Description of studies).

1. Comparison: day patient versus inpatient care for Type 1 trials

1.1 Feasibility and engagement

1.1.1 Proportion of patients suitable for day patient care

We defined the feasibility of day treatment as the percentage reduction in acute inpatient admissions that could be achieved by diverting patients to an acute day hospital (see Methods above). Table 1 summarises the data on the proportion of patients suitable for day hospital treatment. The combined optimistic estimate of feasibility was 37.5% (n = 1768 CI 35.2 to 39.8), whilst the combined pessimistic estimate was 23.2% (n = 2268 CI 21.2 to 25.2). Kallert-EU-2007 reported that 8% to 16% of day hospital patients across the study sites had to be transferred to inpatient settings for clinical reasons.

1.1.2 Number lost to follow-up

Seven trials (Creed-UK-1990; Creed-UK-1996; Dick-UK-1985; Herz-US-1971; Kallert-EU-2007; Schene-NL-1993; Sledge-US-1996) reported data on number lost to follow-up (Analysis 1.1), showing no difference between day hospital and control groups at three months (1 RCT, n = 1117, RR 0.97 CI 0.80 to 1.17), six months (2 RCTs, n = 312, RR 0.83 CI 0.58 to 1.19) and 12 months (5 RCTs, n = 1694, RR 0.94 CI 0.82 to 1.08). The pooled results for follow-up of six months and 12 months, however, showed evidence of heterogeneity (I^2 = 64% and I^2 = 45% respectively). Analysis by year of publication suggested a time dependent effect, with earlier trials

having a higher dropout rate in the day hospital group and later trials having either a similar or a lower drop out rate in the day hospital group.

1.2 Extent of hospital care

1.2.1 Duration of index admission

Four trials (Creed-UK-1990; Creed-UK-1996; Kallert-EU-2007; Sledge-US-1996) reported data that permitted calculation of the duration of index admission (defined as time from first admission to discharge to outpatient care) (Analysis 1.2); in three of these studies individual patient data was provided from the authors (Creed-UK-1990; Creed-UK-1996; Sledge-US-1996). These data showed that patients randomised to day hospital care had a significantly longer index admission (4 RCTs, n = 1582, WMD 27.47 CI 3.96 to 50.98). There was, however, high heterogeneity (I^2 = 91%), which was attributable to differences between the three EU trials (where day patient was significantly longer than in patient stay), and the US trial (where day patient was shorter than inpatient stay). Two further trials (Dick-UK-1985; Herz-US-1971) also reported data on duration of index admission, but in a form that could not be included in the meta-analysis (Analysis 1.3).

1.2.2 Days in inpatient or day patient care

The use of hospital care throughout the study was assessed using individual patient data from three trials (Creed-UK-1990; Creed-UK-1996; Sledge-US-1996) (Analysis 1.4). These data showed no difference in total number of days in hospital between day hospital patients and controls (3 RCTs, n = 465, WMD -0.38 days/month CI -1.32 to 0.55). However, further analyses of these data showed that, compared to controls, patients randomised to day hospital care spent significantly more days in day hospital care (3 RCTS, n = 265, WMD 2.34 days/month CI 1.97 to 2.70; Analysis 1.5) and significantly fewer days in inpatient care (3 RCTs, n = 265, WMD -2.75 days/month CI -3.63 to -1.87; Analysis 1.6).

1.2.3 Readmitted to in/day patient care after discharge

Five trials reported data on number of patients readmitted to hospital care (either inpatient or day hospital) after discharge from the index admission (Creed-UK-1990; Creed-UK-1996; Dick-UK-1985; Herz-US-1971; Sledge-US-1996). These data showed no significant difference between day hospital and control groups (n = 667, RR 0.91 CI 0.72 to 1.15) (Analysis 1.7).

1.3 Clinical and social outcomes

Three trials (Creed-UK-1990; Creed-UK-1996; Sledge-US-1996, total n = 486) provided individual patient data on mental state and social functioning at various time points. Although the trials differed in the choice of questionnaire instruments and time points for follow-up data collection (Table 2), it was possible to combine the individual patient data from the trials. Table 3 gives a breakdown of demographic characteristics of patients from these trials. Forty-two (8.6 %) people had to be dropped from the statistical modelling of outcome due to incomplete covariate data. These appear to be evenly distributed between intervention groups (Table 3). No data were available on quality of life, though one trial had used an unpublished quality of life scale (Sledge-US-1996).

1.4 Mental state (at various time points)

Due to absence of follow-up mental state data, we were unable to include a further 37 patients (7.6%) in this analysis. These were divided between as follows: seven from Creed-UK-1990 (five inpatients and two day patients), seven from Creed-UK-1996 (five inpatients and two day patients) and 23 from Sledge-US-1996 (16 inpatients and seven day patients). There was evidence of curvature of the profiles and positive skew, so we used a square root transformation. The square root transformed profiles were more linear and the patient and time-point level residuals less skewed. There was evidence of both a significant random intercept (Chi^2 = 180.25, P < 0.001) and a significant random slope effect (Chi^2 = 25.46, P < 0.001) measured by change in log-likelihood, so we included both these terms in the statistical modelling. When a full model including time-treatment interaction was compared with a reduced model without the interaction, there was evidence of a significant time-treatment interaction measured by change in log likelihood (Chi^2 = 9.66, P = 0.002). The difference in slope was -0.007 (CI -0.011 to -0.002) with the negative coefficient representing increased improvement in the day hospital group (Table 4).The intervention group had a significant effect (Chi^2 = 4.58, P = 0.032), indicating a difference in baseline levels for the two groups. The difference was 0.144 (CI 0.009 to 0.278), representing a higher baseline for the day hospital group. To ensure that this difference was not causing the difference in slope, we repeated the analysis without this term so forcing a common baseline to be modelled. The overall conclusion did not alter, indicating that the differing baseline values were not causing the significant difference between slopes. None of the other covariates had a significant effect. Unfortunately it is not possible to estimate the extent of the difference in improvement rates, as back transformation of square-root transformed data is not easily interpreted. Dick-UK-1985 (which did not provide individual patient data) also measured mental state using the Clinical Interview Schedule (Goldberg 1972) at 0.75, four and 12 months. No standard deviations were provided, but a significant difference in favour of day hospital treatment was reported at 0.75 months, but not at the other time points (decrease in score: 0.75 ms DP 13.6 IP 9.6, P < 0.001 T test; 4ms DP 16.2 IP 11.6, P = ns; 12 ms DP 20 IP 14.1, P = ns).

1.4.1 Mental state: average endpoint score (BPRS, high=poor)

One trial reported data for mental state (Analysis 1.8) and found that day hospital care was not superior to inpatient care in improving mental state at discharge (n 1 117, MD -0.01 CI -0.07 to 0.05), at three months (n = 1117, MD -0.05 CI -0.11 to 0.01) and at 12 months (n = 1117, MD -0.05 CI -0.11 to 0.01). At admission, the mental state of day hospital patients was more favourable than inpatients (n = 1117, MD -0.08 CI -0.13 to -0.03).

1.5 Social functioning (at various time points)

Due to absence of follow-up social functioning data, we were unable to include 149 patients (30.6%) from Type 1 trials in the analysis of data. These were divided between the studies as follows: 15 from Creed-UK-1990 (nine inpatients and six day patients); 83 from Creed-UK-1996 (43 inpatients and 40 day patients); and 51 from Sledge-US-1996 (32 inpatients and 19 day patients). There was evidence of a significant random intercept (Chi^2 = 62.58, P < 0.001), but no significant random slope effect (Chi^2 = 0.80, P = 0.67) measured by change in log-likelihood, so only the random intercept was included in the statistical modelling. When a full model, including time-treatment interaction, was compared with a reduced model without the interaction, there was no evidence of a time-treatment interaction measured by change in log likelihood (Chi^2 = 0.006, P = 0.941, see Table 5). There was a significant age

(Chi^2 = 7.82, P = 0.005) and a significant gender effect (Chi^2 = 21.95, P < 0.001), with increased age having a positive effect on improvement and males improving less.

1.5.1 Social functioning: average overall role score (GSDS-II, high=poor)

One trial reported data for social functioning (Analysis 1.9) and found that day hospital care was superior to inpatient care in improving social functioning at admission (n = 1117, MD -0.13 CI -0.20 to -0.06), at discharge (n = 1117, MD -0.34 CI -0.48 to -0.20), at three months (n = 1117, MD -0.10 CI -0.19 to -0.01) and at 12 months (n = 1117, MD -0.11 CI -0.19 to -0.03).

1.6 Burden on carers (at various time points)

Two trials reported data on burden on carers (Creed-UK-1990 - 0, 3 & 12 months; Creed-UK-1996 - 0, 0.5, 1, 2, 3, 6 & 12 months - Analysis 1.10), collected using the SBAS Burden Scale (Platt 1981). However, we were unable to include data on burden from Creed-UK-1996 at six and 12 months, as it was available on less than 50% of randomised people. The available data showed no difference in carer burden between day hospital and control groups at two weeks, and one, two three and 12 months, although there were limited data for all time points except three months (where mean difference = -0.59 CI -1.62 to 0.44 i.e. not significant but favouring day hospital treatment).

1.7 Death (suicide/homicide/all causes)

Herz-US-1971 and Kallert-EU-2007 reported on deaths amongst participants (Analysis 1.11) and showed no significant difference between treatment groups (n = 1207, RR 0.18 CI 0.02 to 1.54). Other deaths were acknowledged in some trials, but these data were neither reported in relation to group of randomisation, nor was it possible to derive this information from individual patient data.

1.8 Employed at end of study

Two Type 1 trials (Creed-UK-1996; Kris-US-1965) reported number unemployed (Analysis 1.12), and found a significant difference in favour of day hospital care (2 RCTs, n = 320, RR 0.81, CI 0.67 to 0.97). Creed-UK-1996 provided this data at 12-month follow-up. The data for Kris-US-1965 had limitations as they provided this data on patients two months after discharge, but the duration of the index admission was not specified. They also reported the percentages of the patients that were employed at the end of the study, and as it is unclear the number lost to follow-up from each group, we calculated the number of patients employed based on the total number randomised. Kallert-EU-2007 did not report risk ratios for unemployment, but found that at discharge, those who not reassessed were significantly more likely to be unemployed than those who were reassessed (P < 0.001).

1.9 Satisfaction with care (patients and relatives)

Only Dick-UK-1985 reported data on number not satisfied with care (Analysis 1.13); these data showed a significant difference in favour of day hospital care (n = 91, RR 0.46 CI 0.27 to 0.79, NNT 3). One trial provided score data for treatment satisfaction (Average CAT score, low = poor) (Analysis 1.14) and found that day hospital care was not superior to inpatient care in improving treatment satisfaction at admission (n = 1117, MD 0.22 CI -0.04 to 0.48) and at discharge (n = 1117, MD 0.06 CI -0.18 to 0.30).

1.10 Costs of care

Data on costs of care were reported by four trials (three provided individual patient data) (Analysis 1.15, Analysis 1.16). The four trials found that day hospital care was cheaper than hospital care (with eight of eight comparisons across a range of cost indices favouring day hospital care, six significantly - Analysis 1.16). Reductions in costs ranged from 33.5% to 49.6% for the index admission, to 20.9% to 36.9% for the costs of all psychiatric care (including hospital care). Kallert-EU-2007 also measured costs of care, but this was reported in German. Results from the UK sites were reported in English and found that mean total support costs were higher for the day hospital group over the treatment period: £6523 versus £3619 (bootstrapped 95% CI 375 to 4511). The observed between-group difference for the costs of hospital services (including all inpatient admissions, day hospital attendance and outpatient visits) was large but not statistically significant: £4565 versus £3442 (bootstrapped 95% CI -1185 to 2689).

1.11 Quality of life: average overall role score (MANSA, low=poor)

One trial reported data for quality of life (Analysis 1.17) and found that day hospital care was not superior to inpatient care in improving social functioning at admission (n = 1117, MD -0.02 CI -0.13 to 0.09), at discharge (n = 1117, MD 0.01 CI -0.12 to 0.14), at three months (n = 1117, MD 0.11 CI -0.02 to 0.24) and at 12 months (n = 1117, MD 0.01 CI -0.13 to 0.15).

2. Comparison: day patient versus inpatient care for Type 2 trials

There were two Type 2 trials (Wiersma-NL-1989; Zwerling-US-1964). Only one reported data for seven of the outcomes (Wiersma-NL-1989).

2.1 Feasibility and engagement

2.1.1 Proportion of patients suitable for day patient care

The estimate of feasibility (Table 6) ranged from 18.4% (from Wiersma-NL-1989, which reported the number of people averaging six or more nights per week away from hospital in the first 15 weeks of the trial) to 39.1% (based on Zwerling-US-1964, a trial which reported the number of patients treated entirely in the day hospital without readmission).

2.1.2 Number lost to follow-up

Wiersma-NL-1989 reported data on number lost to follow-up (Analysis 2.1), showing a significant difference in favour of the day hospital group (n = 160, RR 0.69 CI 0.48 to 0.99, NNT 6).

2.2 Extent of hospital care

2.2.1 Duration of all hospital care

Wiersma-NL-1989 reported data on the extent of hospital care (Analysis 2.2); however this was in a format that could not be easily compared with that from Type1 trials even though individual patient data were available. Rather than reporting days in day hospital or inpatient care, Wiersma-NL-1989 reported "nights in hospital" (defined as number of nights spent in hospital during follow-up) and "nights out of hospital" (defined for the control group as nights on leave from inpatient care, and for the day hospital group as number of nights spent at home whilst in day care). Wiersma-NL-1989 then combined these data to give a total

length of stay in day/inpatient care. Relative to the data from Type 1 trials, the total length of stay as reported by Wiersma-NL-1989 increases the apparent length of day patient care, because there is no adjustment for the fact that patients do not attend day hospital every day of the week. Using this method, Wiersma-NL-1989 found no difference in total number of days in hospital between day hospital patients and controls (n = 160, WMD 1.1 days/month CI -1.57 to 3.77). These data could not be disaggregated into days in inpatient care and days in day hospital.

2.2.2 Readmitted to in/day patient care after discharge

Wiersma-NL-1989 also reported data on number of people readmitted to hospital care (either inpatient or day hospital) after discharge from the index admission (Analysis 2.3). These data showed no significant difference between day hospital and control groups (n = 160, RR 0.93 CI 0.64 to 1.35).

2.3 Mental state (at various time points)

Wiersma-NL-1989 provided individual patient data on mental state at 0,12 and 24 months (Analysis 2.4), which showed no significant difference between treatment and control groups.

2.4 Social functioning (at various time points)

Wiersma-NL-1989 reported data on social functioning (Groningen Social Disabilities Schedule, Wiersma 1988) at zero, 12 and 24 months (Analysis 2.5). No significant differences were found between treatment and control groups on either variable at any time point.

2.5 Death (suicide/homicide/all causes)

Wiersma-NL-1989 found no difference in death rates (Analysis 2.6) between day hospital and control groups (n = 160, RR 0.74, 95% CI 0.17 to 3.18), but confidence intervals were wide.

2.6 Employed at end of study

Wiersma-NL-1989 found no difference in number unemployed at 24 months (Analysis 2.7) (n = 160, RR 0.95 CI 0.87 to 1.04).

2.7 Costs of care

Wiersma-NL-1989 (IPD provided) found no significant difference between day and inpatient care in two comparisons, although the trend favoured inpatient care (Analysis 2.8, Analysis 2.9).

DISCUSSION

Summary of main results

This review updates a previous version (Marshall 2002). A major improvement is the addition of a large EU-multicentre trial (Kallert-EU-2007), which was based on the recommendations of Marshall 2002 - namely:

1. recognised the need for a multi-centre randomised controlled trial to show how far the findings from the present small number of centres can be more widely replicated;
2. made use of the common set of outcome measures used in this review; and
3. took care to report data on mortality and other untoward events and quality of life.

The summary below reflects the outcomes chosen for the Summary of findings for the main comparison, and is considered the main findings of this review for support of evidence-based decision making.

1. Feasibility and engagement: lost to follow-up by one year

It is reasonable to assume that there is no difference between day hospital care and inpatient care for feasibility and engagement; although the quality of the evidence is moderate there was also a moderate level of heterogeneity in the pooled data. This is likely to be because of a single, small trial with very high risk of biased results favouring the day hospital intervention (Sledge-US-1996).

2. Extent of hospital care: duration of index admission and duration of day patient care (adjusted days/month)

There is moderately strong evidence that the duration of index admission is longer for patients in day hospital care than inpatient care. The results are highly heterogeneous, which is largely due to a single study (Sledge-US-1996), which had a high risk of bias and different results from other included trials. (If this trial is removed heterogeneity falls - I^2 7% - and confidence in the result increases (MD 33.98 CI 26.18 to 41.78) but the overall direction and extent of finding is similar.

3. Extent of hospital care: duration of day patient care (adjusted days/month)

The quality of the evidence is very low regarding duration of day patient care (adjusted days/month). The impression is that this is longer for patients in day hospital care than those in inpatient care.

4. Extent of hospital care: readmitted to in/day patient care after discharge

It is reasonable to assume that there is no difference between day hospital care and inpatient care for the being readmitted to in/day patient care after discharge, although the quality of evidence so far is very low.

5. Unemployed (at end of study)

There is some evidence that day hospital is superior to inpatient care regarding unemployment at the end of the study. However, the evidence is of low quality as it comes from only two small studies, both of which had limitations in the study design.

6. Quality of life and treatment satisfaction

It is likely that there is no difference between day hospital care and inpatient care for quality of life and treatment satisfaction. The data for this outcome are only from the most recent large multi-centre trial (Kallert-EU-2007), therefore the quality of the evidence was rated as moderate.

Overall completeness and applicability of evidence

1. Completeness

After taking individual patient data into account, trials provided useable data on all the outcomes defined under Types of outcome measures above.

2. Applicability

A limitation in the applicability of the review is an apparent difference in practice between US and EU day hospitals. Data on duration of index admission (both IPD data and other aggregate data) suggests that US acute day hospitals are geared towards intensive treatment and rapid discharge, whereas EU day hospitals allow a more gradual tailing off of day care. It is unclear how far this difference has implications for effectiveness or cost. Inclusion criteria do not appear to be an important limitation on the applicability of the review. Generally Type 1 trials used similar explicit inclusion criteria (that exclude involuntary, suicidal or dangerous patients), with the exceptions of Kris-US-1965 (which contributed little data to the meta-analysis) and Creed-UK-1996 (which excluded patients with mania).

A limitation of this review is that, although we have some information about costs, a proper cost-effectiveness analysis of day hospital versus inpatient care is missing and would be an important addition to this review.

Quality of the evidence

Evidence is estimated to be of moderate quality (based on GRADE). The additional trial, Kallert-EU-2007, was instigated following the findings of the previous version of this review (Marshall 2002) and tried to encompass relevant outcomes. This was an important addition to the review as it increases the confidence in the results for the outcomes for which it contributed data, although some of the outcomes were measured on different scales and so could not be pooled. This trial was of very low risk of bias and carried more weight than the other pooled trials, and was treated as such when assessing the risk of bias for the measured outcomes. In terms of allocation concealment, the quality of included studies was varied; Creed-UK-1990, Creed-UK-1996, Kallert-EU-2007 and Zwerling-US-1964 were good, but the remaining studies were poor. Whilst no trial used evaluators who were blind to group allocation, due to the nature of the interventions, in all studies for which we obtained individual patient data, the authors confirmed that evaluations were performed by independent evaluators. Follow-up rates were generally sub-optimal, and were below 80% in all trials providing individual patient data. The fact that high attrition rates are common to all recent trials suggests the problem lies in working with an acutely ill study population, rather than reflecting design limitations in any particular trial. It is, however, feasible that there is a problem common to all trial design. There was no evidence of a difference in follow-up rates between treatment and controls in trials providing individual patient data, so it is unlikely that lower attrition rates would have had an impact on the findings of this review; however this possibility cannot be absolutely discounted.

Potential biases in the review process

Significant attempts have been made to avoid bias in the review process: we sought individual patient data from most studies and we did not combine data from Type 1 and Type 2 trials. We are aware that there is still the potential for bias. However, the additional trial included was based on the previous version of this review and took into account in the study design the recommendations of this review. It was not possible to tell if there was publication bias, as there were only 10 included trials and a funnel plot is unreliable in this case.

Agreements and disagreements with other studies or reviews

We do not know of any other relevant quantitative review in this topic.

AUTHORS' CONCLUSIONS

Implications for practice

The evidence is of moderate quality, and there is reasonable evidence to support the use of day hospital care to reduce inpatient care whilst improving patient outcome amongst those suitable for day hospital care, so it is curious that they are not more popular. In part this may be due to the difficulties in interpreting day hospital trials, or the fickleness of psychiatric opinion (see introduction). On the other hand there are three disadvantages of day hospital treatment that need to be considered.

1. Day hospital treatment does not appear to be as effective in reducing admission rates as more radical crisis intervention approaches

For example, Assertive Community Treatment, when used to divert patients from hospital, can achieve a 55% reduction in admissions as against the 23% achieved by day hospitals (see Irving 2010 for a systematic review). However, the fact that acute day hospitals do not involve radical, and perhaps unsustainable, alternations in psychiatric practice (Irving 2010) needs to be considered.

2. Cost savings achieved by day hospital care are at best modest

For example, compared with savings of up to 65% reported in studies of crisis intervention (Marshall 1999), acute day hospital care (taking a pessimistic estimate) can be expected to achieve a saving of 4.8% in the costs of acute psychiatric care (calculated as: cost savings inpatients diverted multiplied by the proportion of patients diverted, i.e. 20.9 x 0.232, assuming no inpatient beds were closed). Moreover the cost equation would appear as yet more unfavourable if it were necessary to build the day hospital, rather than change practice in an existing non-acute day hospital. On the other hand, so far it has proven difficult to reliably quantify exactly how much is saved by crisis intervention approaches (Joy 2000). Moreover, if acute day hospitals proved to be more sustainable than crisis intervention alternatives, this might mean that inpatient beds could actually be closed, thus shifting the cost equation in favour of day hospital care. Future versions of this review will have more information about costs as Kallert-EU-2007 reports on costs in a report written in German, which is yet to be translated.

3. It is not clear where day hospitals fit with other types of care

The third disadvantage is that whilst more recent trials (Creed-UK-1996; Kallert-EU-2007; Sledge-US-1996) have enhanced day hospital care with respite or outreach services, it still remains unclear how day hospital care fits together with other types of community care, such as Assertive Community Treatment or home-based care.

In summary therefore, the decision to establish an acute day hospital must be made after careful consideration of local problems and resources. Acute day hospitals are an attractive option in situations where demand for inpatient care is high and facilities exist that are suitable for conversion. They are a less attractive

option in situations where the demand for inpatient care is low and where effective alternatives are already in operation. The inclusion of a large, multicentre trial (Kallert-EU-2007) has reinforced the findings that day hospital care is as effective as inpatient care in treating acutely ill psychiatric patients.

Implications for research

1. Methodological implications for research on acute day hospitals

1.1 A multi-centre randomised controlled trial was called for in the previous version of this review. This clinical trial was performed and reinforces the results about feasibility, days in hospital and has provided unique data on quality of life and treatment satisfaction.

1.2 Although there is data on costs, and we are awaiting the translation of costs data for Kallert-EU-2007, it is likely that more new data on cost-effectiveness are needed.

2. New directions for acute day hospital research

2.1 It would be of interest to explore the relative cost effectiveness of the US and UK approaches to acute day hospital care (rapid discharge versus gradual discharge).

2.2 It would be interesting to examine why patients' psychiatric symptoms appear to recover more rapidly in day care (for example, does hospital admission actually worsen symptoms of depression or anxiety?).

2.3 It is important to examine how acute day hospital care can be most effectively integrated into a modern community based psychiatric service.

ACKNOWLEDGEMENTS

The authors would like to thank Barbara Tomenson and Jacob Tebes for their help in preparing individual patient data; Peter Tyrer for attending the contributors' meeting and Peter Tyrer and Stefan Priebe for providing comments on the methodology for the first version of this review (Marshall 2002).

We would also like to acknowledge and thank the following for their contributions to the original review:

Durk Wiersma - prepared and provided individual patient data, provided additional information about their trials as requested, advised on interpretation of data and the final report.

Chris Roberts - designed and carried out the analysis of individual patient data for mental state and social functioning, advised on interpretation of data and the final report.

Eliz Hill - designed and carried out the analysis of individual patient data for mental state and social functioning and the final report.

Francis Creed - conceived the review and obtained funding, designed the review, prepared and provided individual patient data, provided additional information about their trials as requested, advised on interpretation of data and the final report.

Anna Almaraz-Serrano - designed the review, co-ordinated the review and collected the data, developed, ran and screened the results of the search strategy, organised the retrieval of papers, appraised papers and extracted data, and advised on the final report.

Herman Kluiter - prepared and provided individual patient data, provided additional information about their trials as requested, advised on the assessment of feasibility, advised on interpretation of data, and the final report.

The 2011 update of this review used some text for the Methods section that is generic and created by the Cochrane Schizophrenia Group for use across all reviews. This text has been adapted for use in this review.

REFERENCES

References to studies included in this review

Creed-UK-1990 *{published data only}*

* Creed F, Black D, Anthony P, Osborn M, Thomas P, Tomenson B. Randomised controlled trial of day patient versus inpatient psychiatric treatment. *BMJ* 1990;**300**:1033-7.

Creed-UK-1996 *{published data only}*

Creed F. Cost benefit study of day care - the effect of an attached community psychiatric nurse. Proceedings of the 8th Congress of the Association of European Psychiatrists; 1996 Jul 7-12; London, UK. 1996.

* Creed F, Mbaya P, Lancashire S, Tomenson B, Williams B, Holme S. Cost effectiveness of day and inpatient psychiatric treatment. *BMJ* 1997;**314**:1381-5.

Dick-UK-1985 *{published data only}*

* Dick P, Cameron L, Cohen D, Barlow, Ince A. Day and full time psychiatric treatment: a controlled comparison. *British Journal of Psychiatry* 1985;**147**:246-9.

Dick P, Ince A, Barlow M. Day treatment: suitability and referral procedure. *British Journal of Psychiatry* 1985;**147**:250-3.

Herz-US-1971 *{published data only}*

* Herz MI, Endicott J, Spitzer RL, Mesnikoff A. Day versus inpatient hospitalization: a controlled study. *American Journal of Psychiatry* 1971;**10**:1371-82.

Kallert-EU-2007 *{published data only}*

Adamowski T, Hadrys T, Kiejna A. Comparison between day-care ward and inpatient ward in terms of treatment effectiveness based on the analysis of psychopathologic symptoms, subjective quality of life and number of rehospitalisations after discharge. *Archives of Psychiatry and Psychotherapy* 2009;**11**(3):67-73.

Adamowski T, Hadrys T, Kiejna A. Comparison between the day-care ward and the inpatient ward in terms of treatment effectiveness based on the analysis of psychopathologic symptoms, subjective quality of life and number of rehospitalisations after discharge [Efektywnosc leczenia na psychiatrycznym oddziale dziennym, w porownaniu z oddzialem stacjonarnym, na podstawie analizy objawow psychopatologicznych, subiektywnej oceny jakosci zycia oraz czestosci rehospitalizacji po zakonczeniu leczenia]. *Psychiatria Polska* 2008;**42**(4):571-81. [MEDLINE: 19189601]

Kallert TW. Psychiatric day hospital treatment. http://www.clinicaltrials.gov 2005.

Kallert TW, Matthes C, Glöckner M, Eichler T, Koch R, Schützwohl M. Acute psychiatric day hospital treatment: is the effectiveness of this treatment approach still questionable?. *Psychiatrische Praxis* 2004;**31**(8):409-19.

* Kallert TW, Priebe S, McCabe R, Kiejna A, Rymaszewska J, Nawka P, Ocvar L, Raboch J, Starkova-Kalisova L, Koch R, Schutzwohl M. Are day hospitals effective for acutely ill psychiatric patients? A European multicentre randomized controlled trial. *Journal of Clinical Psychiatry* 2007;**68**(2):278-87. [MEDLINE: 17335327]

Kallert TW, Schonherr R, Frohling D, Schutzwohl M. Comparing individual costs for treatment in acute day and inpatient care--results from a randomised controlled trial. *Psychiatrische Praxis* 2007;**34**(8):377-83.

Kallert TW, Schonherr R, Schnippa S, Matthes C, Glockner M, Schutzwohl M. Direct costs of acute day hospital care: results from a randomized controlled trial. *Psychiatrische Praxis* 2005;**32**:132-41.

Kallert TW, Schtzwohl M, Priebe S, Kiejna A, Raboch J, Nawka P. Quality assurance for randomisation and survey instruments in the Eden-study. *European Psychiatry* 2002;**17**(Suppl 1):58s.

Kiejna A, Kallert TW, Rymaszewska J. Treatment in psychiatric day hospital in comparison with inpatient wards in different European health care system - objectives of Eden project. *Psychiatria Polska* 2002;**36**(6):361-7.

Kiejna A, Szajowski K, Rymaszewska J, Jarosz-Nowak J, Kallert T. Psychopathology and social disabilities of day-hospital and inpatients during acute treatment. *European Psychiatry* 2002;**17**(Suppl 1):59s.

Okunade TA, Priebe S, Kiejna A, Raboch J, Nawka P. Eden: European day hospital study in five European centres. Proceedings of the 12th World Congress of Psychiatry; 2002 Aug 24-29; Yokohama, Japan. 2002.

Priebe S. Rct evaluating a new psychiatric day hospital for acute treatment: one year follow up. http://www.controlled-trials.com 2004.

Priebe S, Jones G, McCabe R, Briscoe J, Wright D, Sleed M, Beecham J. Effectiveness and costs of acute day hospital treatment compared with conventional in-patient care: randomised controlled trial. *British Journal of Psychiatry* 2006;**188**:243-9.

Raboch J. Experiences with Eden project and day care system at the psychiatric department in Prague. Proceedings of the 12th World Congress of Psychiatry; 2002 Aug 24-29; Yokohama, Japan. 2002.

Rymaszewska J, Kiejna A, Jarosz J. Quality of life and assessment of acute day vs inpatient treatment. *European Psychiatry* 2002;**17**(Suppl 1):198s.

Rymaszewska J, Kiejna A, Jarosz-Nowak J. Quality of life and social functioning of day hospital and inpatients. Proceedings of the 3rd International Conference on Early Psychosis; 2002 Sep 25-28; Copenhagen, Denmark. 2002.

Schtzwohl M, Kallert TW, Matthes C. Burden on relatives comparing treatment in inpatient and day-hospital settings. *European Psychiatry* 2002;**17**(Suppl 1):59s.

Schutzwohl M, Koch R, Kallert TW. On the efficacy of acute psychiatric day-care treatment in a one-year-follow-up. A

comparison to inpatient treatment within a randomised controlled trial. *Psychiatrische Praxis* 2007;**34**(1):34-7. [MEDLINE: 17106839]

Schützwohl M, Glöckner M, Matthes C, Eichler T, Kallert T. The burden on relatives of acute mentally ill within the first four weeks of day hospital and inpatient treatment. Results from a randomized controlled trial [Die belastung von bezugspersonen voll- und teilstationär behandelter psychisch erkrankter--ergebnis einer randomisierten kontrollierten untersuchung]. *Psychiatrische Praxis* 2005;**32**(6):281-8.

Schützwohl M, Kallert TW, Jurjanz L. Dimensional diagnostics using the schedules for clinical assessment in neuropsychiatry (scan 2.1). Findings from a cross-national multi-site study. Proceedings of the XIII World Congress of Psychiatry; 2005 Sep 10-15; Cairo, Egypt. 2005.

Starkova L, Raboch J, Kitzlerova E, Bakova N, Sekot M, Howardova A, Peichlova J, Kallert T. Evaluation of clinical state, treatment satisfaction and subjective life quality in two groups of patients in day care treatment on conventional impatient treatment - the international EDEN study (final results). *Ceska a Slovenska Psychiatrie* 2003;**99**(5):260-4.

Kris-US-1965 *{published data only}*

* Kris EB. Day hospitals. *Current Therapeutic Research* 1965;**7**:320-3.

Schene-NL-1993 *{published data only}*

* Schene AH, van Wijngaarden B, Poelijoe NW, Gersons BPR. The Utrecht comparative study on psychiatric day treatment and inpatient treatment. *Acta Psychiatrica Scandinavica* 1993;**87**:427-36.

Sledge-US-1996 *{published data only}*

Davidson L, Tebes JK, Rakfeldt J, Sledge WH. Differences in social environment between inpatient and day hospital-crisis respite settings. *Psychiatric Services* 1996;**47**:714-20.

* Sledge WH, Tebes J, Rakfeldt J, Davidson L, Lyons L, Druss B. Day hospital/crisis respite care versus inpatient care, part I: clinical outcomes. *American Journal of Psychiatry* 1996;**153**:1065-73.

Sledge WH, Tebes J, Wolff N, Helminiak TW. Day hospital/crisis respite care versus inpatient care, part II: service utilization and costs. *American Journal of Psychiatry* 1996;**153**:1074-83.

Wiersma-NL-1989 *{published data only}*

Kluiter H. Day treatment for unselected patients referred for inpatient psychiatric treatment: results of a randomised trial. Een verschil van dag en nacht? Resultaten van een gerandomiseerd onderzoek naar de vervangbaarheid van psychiatrische opname door dagbehandeling. Univ. of Groningen, 1993.

Kluiter H, Giel R, Nienhuis F, J, Ruephan M. Feasibility of a day-treatment program for the severely mentally ill: first results of a randomized experiment [Haalbaarheid van dagbehandeling voor de opnamepopulatie van een algemeen psychiatrisch ziekenhuis. Berichten vanuit het Substitutieproject Drenthe (II)]. *Tijdschrift voor Psychiatrie* 1990;**32**(9):625-39.

Kluiter H, Giel R, Nienhuis FJ, Ruphan M, Wiersma D. Predicting feasibility of day treatment for unselected patients referred for inpatient psychiatric treatment: results of a randomized trial. *American Journal of Psychiatry* 1992;**149**:1199-205.

Nienhuis F, Giel R, Kluiter H, Ruephan M. Feasibility of a day-treatment program for the severely mentally ill in relation to psychopathology: IV [Haalbaarheid van opnamevervangende dagbehandeling in relatie tot de psychopathologie bij opname. Berichten uit het substitutieproject drenthe (IV)]. *Tijdschrift voor Psychiatrie* 1991;**33**(7):463-76.

Nienhuis FJ, Giel R, Kluiter H, Ruphan M, Wiersma D. Efficacy of psychiatric day treatment. Course and outcome of psychiatric disorders in a randomised trial. *European Archives of Psychiatry and Clinical Neuroscience* 1994;**244**:73-80.

Ruephan M, Giel R, Kluiter H, Nienhuis F. Feasibility of a day-treatment program for the severely mentally ill in relation to social role dysfunction [Haalbaarheid van opnamevervangende dagbehandeling in relatie tot sociale beperkingen bij opname. Berichten uit het substitutieproject Drenthe (V)]. *Tijdschrift voor Psychiatrie* 1991;**33**(7):477-89.

Ruphan M, Kluiter H, Nienhuis FJ, Wiersma D, Giel R. Day-treatment versus inpatient treatment for psychiatric patients in need of admission: longitudinal effects on social role-functioning studied by means of randomized trial [Opnamevervangende dagbehandeling bij psychiatrische patienten: Effecten op het sociale functioneren, twee jarr na de start van een gecontroleerd experiment]. *Tijdschrift voor Psychiatrie* 1992;**34**:571-83.

Wierma D, Kluiter H, Nienhuis F, Ruphan M, Giel R. Day-treatment with community care as an alternative to standard hospitalization: an experiment in the Netherlands. A preliminary communication. Groningen, Netherlands: Department of Social Psychiatry, University of Groningen, 1989.

* Wiersma D, Kluiter H, Nienhuis FJ, Ruphan M, Giel R. Costs and benefits of day treatment with community care for schizophrenic patients. *Schizophrenia Bulletin* 1991;**3**:411-9.

Wiersma D, Kluiter H, Nienhuis FJ, Ruphan M, Giel R. Costs and benefits of hospital and day treatment with community care of affective and schizophrenic disorders. *British Journal of Psychiatry* 1995;**166**:52-9.

Zwerling-US-1964 *{published data only}*

Wilder JF, Levin G, Zwerling I. A two-year follow-up evaluation of acute psychotic patients treated in a day hospital. *American Journal of Psychiatry* 1966;**122**:1095-101.

* Zwerling I, Wilder JF. An evaluation of the applicability of the day hospital in the treatment of acutely disturbed patients. *Israel Annals of Psychiatry and Related Disciplines* 1964;**2**:162-85.

References to studies excluded from this review

Austin-Los Angeles *{published data only}*

Austin NK, Liberman RP, King LW, DeRisi WJ. A comparative evaluation of two day hospitals. Goal attainment scaling of

behaviour therapy vs. milieu therapy. *Journal of Nervous and Mental Disease* 1976;**163**:253-62.

Azim-Alberta *{published data only}*

Azim HF, Weiden TD, Ratcliffe WD, Nutter RW, Dyck RJ, Howarth BG. Current utilization of day hospitalization. *Canadian Psychiatric Association Journal* 1978;**23**:557-66.

Barkley-Ontario *{published data only}*

Barkley AL, Fagen K, Lawson JS. Day care: can it prevent readmission to a psychiatric hospital?. *Psychiatric Journal of the University of Ottawa* 1989;**14**:536-41.

Basker-Jerusalem *{published data only}*

Basker E, Turel M. The day hospital: a comparative study of an alternative to full psychiatric hospitalization. *Israeli Journal of Psychiatry and Related Sciences* 1986;**23**:287-96.

Bateman-London *{published data only}*

Bateman A, Fonaghy P. Effectiveness of partial hospitalization in the treatment of borderline personality disorder: a randomized controlled trial. *American Journal of Psychiatry* 1999;**156**(10):1563-9.

Beigel-New York *{published data only}*

Beigel A, Feder SL. Patterns of utilization in partial hospitalization. *American Journal of Psychiatry* 1970;**126**:101-8.

Bertrand-Belgium *{published data only}*

Bertrand J, Lisin P. Experiment with hospital day care in Belgium. *Acta Psychiatrica Belgica* 1973;**73**(1):53-65.

Boath-Stoke *{published data only}*

Boath E, Cox J, Lewis M, Jones P, Pryce A. When the cradle falls: the treatment of postnatal depression in a psychiatric day hospital compared with routine primary care. *Journal of Affective Disorders* 1999;**53**(2):143-51.

Bowman-Dublin *{published data only}*

Bowman EP, Shelley RK, Sheehy-Skeffington A, Sinanan K. Day patient versus in-patient: factors determining selection of acutely ill patients for hospital treatment. *British Journal of Psychiatry* 1983;**42**:584-7.

Bradshaw-Minnesota *{published data only}*

Bradshaw W. Integrating cognitive-behavioral psychotherapy for persons with schizophrenia into a psychiatric rehabilitation program: results of a three year trial. *Community Mental Health Journal* 2000;**36**(5):491-500.

Brook-Denver *{published data only}*

Brook BD. Crisis hostel: an alternative to psychiatric hospitalization for emergency patients. *Hospital & Community Psychiatry* 1973;**24**:621-4.

Carey-US *{published data only}*

Carey MP, Carey KB, Meisler AW. Training mentally ill chemical abusers in social problem solving. *Behavior Therapy* 1990;**21**(4):511-19.

Case-New York *{published data only}*

Case N. The dual-diagnosis patient in a psychiatric day treatment program: a treatment failure. *Journal of Substance Abuse Treatment* 1991;**8**:69-73.

Comstock-Texas *{published data only}*

Comstock BS, Kamilar SM, Thornby JI, Ramirez JP, Kaplan HB. Crisis treatment in a day hospital. Impact on medical care-seeking. *Psychiatric Clinics of North America* 1985;**8**:483-500.

Creed-Blackburn *{published data only}*

Creed F, Black D, Anthony P, Osborn M, Thomas, Franks D, Polley R, Lancashire S, Saleem P, Tomenson B. Randomised controlled trial of day and in-patient psychiatric treatment. 2: Comparison of two hospitals. *British Journal of Psychiatry* 1991;**158**:183-9.

Creed-Manchester *{published data only}*

Creed F, Anthony P, Godbert K, Huxley P. Treatment of severe psychiatric illness in a day hospital. *British Journal of Psychiatry* 1989;**154**:341-7.

Dal Santo 2004 *{published data only}*

Dal Santo B, Luoni A, Corbetta A, Scarone S. Uses and advantages of day hospital in psychiatric patients: an alternative way of care. *Schizophrenia Research* 2004;**67**(1):150-1.

Davidson 2006 *{published data only}*

Davidson L, Tebes JK, Rakfeldt J, Sledge WH. Differences in social environment between inpatient and day hospital-crisis respite settings. *Psychiatric Services* 1996;**47**:714-20.

Dick-Dundee *{published data only}*

Dick PH, Sweeney ML, Crombie IK. Controlled comparison of day-patient and out-patient treatment for persistent anxiety and depression. *British Journal of Psychiatry* 1991;**158**:24-7.

Drake-New Hampshire *{published data only}*

Drake RE, Becker DR, Biesanz JC, Torrey WC, McHugo GJ, Wyzik PF. Rehabilitative day treatment vs. supported employment: I. vocational outcomes. *Community Mental Health Journal* 1994;**30**:519-32.

Ettlinger-New York *{published data only}*

Ettlinger RA, Beigel A, Feder SL. The partial hospital as a transition from inpatient treatment: a controlled follow-up study. *Mount Sinai Journal of Medicine* 1972;**39**:251-7.

Fink-Toronto *{published data only}*

Fink EB, Longbaugh R, Stout R. The paradoxical underutilization of partial hospitalization. *American Journal of Psychiatry* 1978;**135**:713-6.

Glick-New York *{published data only}*

Glick ID, Fleming L, DeChillo N, Meyerkopf N, Jackson C, Muscara D, Good-Ellis M. A controlled study of transitional day care for non-chronically-ill patients. *American Journal of Psychiatry* 1986;**143**:1551-6.

Glick-San Francisco *{published data only}*

Glick ID, Hargreaves WA, Drues JA, Showstack JA, Katzow JJ. Short versus long hospitalization: a prospective controlled study. VII. Two-year follow-up results for non-schizophrenics. *Archives of General Psychiatry* 1977;**34**:314-7.

Glick ID, Hargreaves WA, Drues JMA, Showstack JA. Short versus long hospitalization: a controlled study. III. Inpatient results for non-schizophrenics. *Archives of General Psychiatry* 1976;**33**:78-83.

Glick ID, Hargreaves WA, Drues JMA, Showstack JA. Short versus long hospitalization: a prospective controlled study. IV. One-year follow-up results for schizophrenic patients. *American Journal of Psychiatry* 1976;**133**:509-14.

Glick ID, Hargreaves WA, Drues JMA, Showstack JA. Short versus long hospitalization: a prospective controlled study. V. One-year follow up for non-schizophrenics. *American Journal of Psychiatry* 1976;**133**:515-7.

Glick ID, Hargreaves WA, Goldfield MD. Short vs long hospitalization. A prospective controlled study. I. The preliminary results of a one-year follow-up for schizophrenics. *Archives of General Psychiatry* 1974;**30**:363-9.

Glick ID, Hargreaves WA, Raskin M, Kutner SJ. Short versus long hospitalization: a prospective controlled study. II. Results for schizophrenic inpatients. *American Journal of Psychiatry* 1975;**132**(4):385-90.

Hargreaves WA, Glick ID, Drues J, Showstack JA, Feigenbaum E. Short versus long hospitalization: a prospective controlled study. VI. Two-year follow-up results for schizophrenics. *Archives of General Psychiatry* 1977;**34**:305-11.

Grad-Chichester *{published data only}*

Grad J, Sainsbury P. The effects that patients have on their families in a community care and a control psychiatric service - a two year follow-up. *British Journal of Psychiatry* 1968;**114**:265-78.

Gudeman-Boston *{published data only}*

Gudeman JE, Shore MF, Dickey B. Day hospitalization and an inn instead of inpatient care for psychiatric patients. *New England Journal of Medicine* 1983;**308**:749-53.

Guidry-New Orleans *{published data only}*

Guidry LS, Winstead DK, Levine M, Eicke FJ. Evaluation of treatment centre effectiveness. *Journal of Clinical Psychiatry* 1979;**40**:221-4.

Guillette-Maryland *{published data only}*

Guillette W, Crowley B, Savitz S, Goldberg FD. Day hospitalization as a cost-effective alternative to inpatient care: a pilot study. *Hospital & Community Psychiatry* 1978;**29**:525-7.

Guy-Baltimore *{published data only}*

Guy W, Gross M, Hogarty GE, Dennis H. A controlled evaluation of day hospital effectiveness. *Archives of General Psychiatry* 1969;**20**:329-38.

Hogarty GE, Dennis H, Guy W, Gross GM. Who goes there? A critical evaluation of admissions to a psychiatric day hospital. *American Journal of Psychiatry* 1968;**124**:934-44.

Herz-New York2 *{published data only}*

Herz MI, Endicott J, Spitzer RL. Brief hospitalization of patients with families: initial results. *American Journal of Psychiatry* 1975;**132**:413-8.

Hirsch-London *{published data only}*

Hirsch SR, Platt S, Knights A, Weyman A. Shortening hospital stay for psychiatric care: effect on patients and their families. *British Medical Journal* 1979;**1**:442-6.

Hogg-Glasgow *{published data only}*

Hogg LI, Brooks N. New chronic schizophrenic patients: a comparison of daypatients and inpatients. *Acta Psychiatrica Scandinavica* 1990;**81**:271-6.

Inch-Saskatchewan *{published data only}*

Inch R, Crossley M, Keegan D, Thorarinson D. Use of the brief psychiatric rating scale to measure success in a psychosocial day program. *Psychiatric Services* 1997;**48**(9):1195-7.

Jarema-Warsaw *{published data only}*

Jarema M, Bury L, Konieczynska Z, Zaborowski B, Cikowska G, Kunicka A, Bartoszewicz J, Muraszkiewicz L. Comparison of quality of life of schizophrenic patients in different forms of psychiatric care. *Psychiatria Polska* 1997;**31**(5):585-94.

Kandel-US *{published data only}*

Kandel A, Langrod J, Ruiz P. Changes in future time perception of day hospital psychiatric patients in response to small group treatment approaches. *Journal of Clinical Psychology* 1981;**37**(4):769-81.

Kecmanovic-Sarajevo *{published data only}*

Kecmanovic D. Post release adjustment of day and inpatients. *International Journal of Social Psychiatry* 1985;**31**:74-9.

Klyczek-US *{published data only}*

Klyczek JP, Mann WC. Therapeutic modality comparisons in day treatment. *American Journal of Occupational Therapy* 1986;**40**(9):606-11.

Konieczynska-Warsaw *{published data only}*

Konieczynska Z, Pietrzykowska B, Zaborowski B. Family burden of patients receiving inpatient treatment or alternative forms of psychiatric care. *Psychiatria Polska* 1997;**31**(5):625-35.

Konieczynska Z, Pietrzykowska B, Zaborowski B. Follow-up studies of psychotic patients receiving inpatient treatment or alternative forms of psychiatric care. *Psychiatria Polska* 1997;**31**(5):615-24.

Kuldau-California *{published data only}*

Kuldau JM, Stanley J, Dirks JD. Controlled evaluation of a hospital-originated community transitional system. *Archives of General Psychiatry* 1977;**34**:1331-40.

Levenson-Houston *{published data only}*

Levenson AJ, Lord CJ, Sermas CE, Thornby JI, Sullender W, Comstock BS. Acute schizophrenia: an efficacious outpatient treatment approach as an alternative to full-time hospitalization. *Diseases of the Nervous System* 1977;**38**:242-5.

Liang-Taipei *{published data only}*

Liang Y, Chiang S, Lee T, Wu M, Yan H, Tsui H. A study of the objective life quality among chronic mentally ill patients. *Nursing Research* 1997;**5**(3):212-22.

Linn-USA *{published data only}*

Linn MW, Caffey EM, Klett CJ, Hogarty GE, Lamb HR. Day treatment and psychotropic drugs in the aftercare of schizophrenic patients. *Archives of General Psychiatry* 1979;**36**:1055-66.

Lystad-Louisiana *{published data only}*

Lystad MH. Day hospital care and changing family attitudes toward the mentally ill. *Journal of Nervous and Mental Disease* 1958;**127**:145-52.

Mathai-Bangalore *{published data only}*

Mathai PJ, Gopinath PS. Deficits of chronic schizophrenia in relation to long-term hospitalisation. *British Journal of Psychiatry* 1985;**148**:509-16.

McDonnell-Ireland *{published data only}*

McDonnell D. An evaluation of day centre care. *International Journal of Social Psychiatry* 1977;**23**(2):110-9.

Meltzoff-New York *{published data only}*

Meltzoff J, Blumenthal RL. The Day Treatment Center: Principles, Application and Evaluation. Springfield, Illinois: Charles C Thomas, 1966.

Michaux 1969 *{published data only}*

Michaux MH, Garmize K, Rossi JA, Schoolman LR, Gross GM. A controlled comparison of psychiatric day centre treatment with full time hospitalization. *Current Therapeutic Research, Clinical and Experimental* 1969;**11**(4):190-204.

Michaux-Maryland *{published data only}*

Michaux MH, Chelst MR, Foster SA, Pruim RJ. Day and full-time psychiatric treatment: a controlled comparison. *Current Therapeutic Research* 1972;**14**:279-92.

Milne-Wakefield *{published data only}*

Milne D. A comparative evaluation of two psychiatric day hospitals. *British Journal of Psychiatry* 1984;**145**:533-7.

Newton-US *{published data only}*

Newton PA. An evaluation of the cost effectiveness of day hospitalization for black male schizophrenics. *Journal of the National Medical Association* 1983;**75**(3):273-85.

Niskanen-Helsinki *{published data only}*

Niskanen P. Treatment results achieved in psychiatric day care: a follow-up of 100 patients. *Acta Psychiatrica Scandinavica* 1974;**50**:401-9.

O'Shea-Ireland *{published data only}*

O'Shea E, Hughes J, Fitzpatrick L, Dunne E, O'Sullivan M, Cole M. An economic evaluation of inpatient treatment versus day hospital care for psychiatric patients. *Irish Journal of Psychological Medicine* 1998;**15**(4):127-30.

Odenheimer-USA *{published data only}*

Odenheimer JF. Day hospital as an alternative to the psychiatric ward. Attitudes and responses of relatives. *Archives of General Psychiatry* 1965;**13**:46-53.

Oka-Kurume-Japan *{published data only}*

Oka K, Maeda M, Hirano T, Funahashi T, Yamada S. Multicentre study on the effects of day care therapy on schizophrenia: a comparison of day care patients with outpatients. *Psychiatry & Clinical Neurosciences* 1999;**53**(4):505-10.

Pang-US *{published data only}*

Pang J. Partial hospitalization: an alternative to inpatient care. *Psychiatric Clinics of North America* 1985;**8**:587-95.

Penk-Dallas *{published data only}*

Penk WE, Charles HL, Van Hoose TA. Comparative effectiveness of day hospital and inpatient psychiatric treatment. *Journal of Consulting and Clinical Psychology* 1978;**1**:94-101.

Piersma-Michigan *{published data only}*

Piersma HL, Boes JL. Comparison of psychiatric day hospital patient and inpatient scores on the MCMI-III. *Journal of Clinical Psychology* 1997;**53**(6):629-34.

Piper-Alberta *{published data only}*

Piper WE, Joyce AS, Azim HFA, Rosie JS. Patient characteristics and success in day treatment. *Journal of Nervous and Mental Disease* 1994;**182**:381-6.

Piper WE, Rosie JS, Azim HFA, Joyce AS. A randomized trial of psychiatric day treatment for patients with affective and personality disorders. *Hospital and Community Psychiatry* 1993;**44**:757-63.

Platt-London *{published data only}*

Platt SD, Knights AC, Hirsch SR. Caution and conservatism in the use of a psychiatric day hospital: evidence from a research project that failed. *Psychiatry Research* 1980;**3**:123-32.

Prior-Middlesex *{published data only}*

Prior S. Determining the effectiveness of a short-term anxiety management course. *British Journal of Occupational Therapy* 1998;**61**(5):207-13.

Russell-Ottawa *{published data only}*

Russell V, Mai F, Busby K, Attwood D, Davis M, Brown M. Acute day hospitalization as an alternative to inpatient treatment. *Canadian Journal of Psychiatry* 1996;**41**(10):629-37.

Sandell-Stockholm *{published data only}*

Sandell R, Alfredsson E, Berg M, Crafoord K. Clinical significance of outcome in long-term follow-up of borderline patients at a day hospital. *Acta Psychiatrica Scandinavica* 1993;**87**(6):405-13.

Shek 2009 *{published data only}*

Shek E, Stein AT, Shansis FM, Marshall M, Crowther R, Tyrer P. Day hospital vs outpatient care for people with schizophrenia. *Schizophrenia Bulletin* 2009;**35**(6):1057-8.

Skoda-Czech Republic *{published data only}*

Skoda C, Tomasek L, Bouchal M, Baudis P, Synkova J. A comparison of the effectiveness of three therapeutic preventive programs for neurotics. IV. Risk index for an unfavorable prognosis of the neurosis. *Ceskoslovenska Psychiatrie* 1983;**79**(2):131-7.

Tam-Hong Kong *{published data only}*

Tam SF, Ip YC, Chan CS. Self-concepts of hospitalized and day-care psychiatric patients. *Journal of Social Psychology* 2000;**140**(2):202-9.

Tantam-Manchester *{published data only}*

Tantam D, McGrath G. Psychiatric day hospitals - another route to institutionalization?. *Social Psychiatry and Psychiatric Epidemiology* 1989;**24**:96-101.

Tsukahara 1998 *{published data only}*

Tsukahara T, Takano Y, Kato M, Kasahara T. The effect of day care treatment on duration of subsequent rehospitalization. *Seishin Igaku* 1998;**40**(10):1095-7.

Tyrer-Southampton *{published data only}*

Tyrer P, Remington M, Alexander J. The outcome of neurotic disorders after out-patient and day hospital care. *British Journal of Psychiatry* 1987;**151**:57-62.

Tyrer PJ, Remington M. Controlled comparison of day-hospital and outpatient treatment for neurotic disorders. *Lancet* 1979;**i**:1014-6.

Vaglum-Oslo *{published data only}*

Vaglum P, Friis S, Irion T, Johns S, Karterud S, Larsen F, Vaglum S. Treatment response of severe and nonsevere personality disorders in a therapeutic community day unit. *Journal of Personality Disorders* 1990;**4**(2):161-72.

Vaitl-Haar-Germany *{published data only}*

Vaitl P, Bender W, Hubmann W, Hummel C, Kroner A, Kroner M. Full inpatient versus partial inpatient psychiatric after-care - a comparative retrospective study. *Psychiatrische Praxis* 1989;**16**(6):214-7.

Van Den Hout-NL *{published data only}*

Van Den Hout JH, Arntz A, Kunkels FH. Efficacy of a self-control therapy program in a psychiatric day-treatment centre. *Acta Psychiatrica Scandinavica* 1995;**92**(1):25-9.

Washburn-Boston *{published data only}*

Vannicelli M, Washburn S, Scheff BJ, Longabaugh R. Comparison of usual and experimental patients in a psychiatric day centre. *Journal of Consulting and Clinical Psychology* 1978;**46**:87-93.

Washburn S, Vannicelli M, Longabaugh R, Scheff BJ. A controlled comparison of psychiatric day treatment and inpatient hospitalization. *Journal of Consulting and Clinical Psychology* 1976;**44**:665-75.

Washburn SL, Vannicelli M, Scheff BJ. Irrational determinants of the place of psychiatric treatment. *Hospital and Community Psychiatry* 1976;**27**:179-82.

Weissert 1980 *{published data only}*

Weissert W, Wan T, Livieratos B, Katz S. Effects and costs of day-care services for the chronically ill. A randomized experiment. *Medical Care* 1980;**18**(6):567-84. [MEDLINE: 6772889]

Welburn-Ottawa *{published data only}*

Welburn K, Dagg, P, Coristine M, Pontefract A. Schematic change as a result of an intensive group-therapy day treatment program. *Psychotherapy* 2000;**37**(2):189-95.

Weldon-New York *{published data only}*

Weldon E, Clarkin J, Hennessy JJ, Frances A. Day hospital versus outpatient treatment: a controlled study. *Psychiatric Quarterly* 1979;**51**:144-50.

Wilberg-Oslo *{published data only}*

Wilberg T, Friis S, Karterud S, Mehlum L, Urnes O, Vaglum P. Outpatient group psychotherapy: a valuable continuation treatment for patients with borderline personality disorder treated in a day hospital? A 3-year follow-up study. *Nordic Journal of Psychiatry* 1998;**52**(3):213-22.

Wu 1995 *{published data only}*

Wu Y, Liu C, Yu L. A controlled study of day treatment effect on chronic schizophrenia. *Chinese Journal of Neurology and Psychiatry* 1995;**28**(4):220-2.

References to studies awaiting assessment

Vietze-Germany *{published data only}*

Vietze G, Lange E, Ficker F, Ost H. Day-night therapy - a necessary step towards better care for mental patients. *Psychiatrie, Neurologie und Medizinische Psychologie* 1976;**28**(5):298-306.

References to ongoing studies

Donnison 2001 *{published data only}*

Donnison J. An evaluation of the clinical effectiveness of cognitive-behavioural rapid stabilisation group therapy in a day hospital setting. National Research Register 2001; Vol. 1.

Gjonbalaj-Marovic 2005 *{published data only}*

Gjonbalaj-Marovic S, Whalen L, Maney M, Smelson DA. A brief community linkage intervention for dually diagnosed individuals. http://www.clinicaltrials.gov 2005.

Additional references

Altman 1996

Altman DG, Bland JM. Detecting skewness from summary information. *BMJ* 1996;**313**:1200.

Asberg 1978

Asberg M, Perris C, Schalling D, Sedvall G. The CPRS - development and applications of a rating scale. *Acta Psychiatrica Scandinavica Supplement* 1978;**271**:5-27.

Audit Comm 1994

Audit Commission. Finding a Place: A Review of Mental Health Services For Adults. London: HMSO, 1994.

Beck 1997

Beck A, Croudace TJ, Singh S, Harrison G. The Nottingham acute bed study: alternatives to acute psychiatric care. *British Journal of Psychiatry* 1997;**170**:247-52.

Bland 1997

Bland JM. Statistics notes. Trials randomised in clusters. *BMJ* 1997;**315**:600.

Boissel 1999

Boissel JP, Cucherat M, Li W, Chatellier G, Gueyffier F, Buyse M, Boutitie F, Nony P, Haugh M, Mignot G. The problem of therapeutic efficacy indices. 3. Comparison of the indices and their use. *Therapie* 1999;**54**(4):405-11.

Cameron 1947

Cameron E. The day hospital. An experimental form of hospitalization for psychiatric patients. *The Modern Hospital* 1947;**69**:60-3.

Creed 1989

Creed F, Black D, Anthony P. Day-hospital and community treatment for acute psychiatric illness: a critical appraisal. *British Journal of Psychiatry* 1989;**154**:300-10.

Deeks 2000

Deeks J. Issues in the selection for meta-analyses of binary data. Abstracts of 8th International Cochrane Colloquium; 2000 Oct 25-28th; Cape Town, South Africa. 2000.

Divine 1992

Divine GW, Brown JT, Frazier LM. The unit of analysis error in studies about physicians' patient care behavior. *Journal of General Internal Medicine* 1992;**7**(6):623-9.

DoH 1996

Department of Health. Health and Personal Social Services Statistics for England. London: HMSO, 1996.

Donner 2002

Donner A, Klar N. Issues in the meta-analysis of cluster randomized trials. *Statistics in Medicine* 2002;**21**:2971-80.

Egger 1997

Egger M, Davey-Smith G, Schneider M, Minder C. Bias in meta-analysis detected by a simple, graphical test. *BMJ* 1997;**315**:629-34.

Elbourne 2002

Elbourne D, Altman DG, Higgins JPT, Curtina F, Worthingtond HV, Vaile A. Meta-analyses involving cross-over trials: methodological issues. *International Journal of Epidemiology* 2002;**31**(1):140-9.

Furukawa 2006

Furukawa TA, Barbui C, Cipriani A, Brambilla P, Watanabe N. Imputing missing standard deviations in meta-analyses can provide accurate results. *Journal of Clinical Epidemiology* 2006;**59**:7-10.

Goldberg 1972

Goldberg D. The Detection of Psychiatric Illness by Questionnaire. London: Oxford University Press, 1972.

Greene 1981

Greene LR, De La Cruz MS. Psychiatric day treatment as an alternative to and transition from full-time hospitalization. *Community Mental Health Journal* 1981;**17**:191-202.

Gulliford 1999

Gulliford MC. Components of variance and intraclass correlations for the design of community-based surveys and intervention studies: data from the Health Survey for England 1994. *American Journal of Epidemiology* 1999;**149**:876-83.

Higgins 2003

Higgins JP, Thompson SG, Deeks JJ, Altman DG. Measuring inconsistency in meta-analyses. *BMJ* 2003;**327**:557-60.

Higgins 2009

Higgins JPT, Green S. Cochrane Handbook for Systematic Reviews of Interventions 5.0.2 [updated September 2009]The Cochrane Collaboration, 2009. Available from www.cochrane-handbook.org.

Hoge 1992

Hoge MA, Davidson L, Leonard Hill W, Turner VE, Ameli R. The promise of partial hospitalization: a reassessment. *Hospital and Community Psychiatry* 1992;**43**:345-54.

Irving 2010

Irving CB, Adams CE, Rice K. Crisis intervention for people with severe mental illnesses. *Cochrane Database of Systematic Reviews* 2006, Issue 4. [DOI: 10.1002/14651858.CD001087.pub3]

Johnstone 2001

Johnstone P, Zolese G. Length of hospitalisation for those with severe mental illness. *Cochrane Database of Systematic Reviews* 2001, Issue 2. [DOI: 10.1002/14651858.CD000384.pub2]

Joy 2000

Joy CB, Adams CE, Rice K. Crisis intervention for people with severe mental illnesses. *Cochrane Database of Systematic Reviews* 2000, Issue 1. [DOI: 10.1002/14651858.CD001087.pub3]

Kay 1986

Kay SR, Opler LA, Fiszbein A. *Positive and negative syndrome scale (PANSS) manual*. Positive and negative syndrome scale (PANSS) manual. North Tonawanda (NY): Multi-Health Systems, 1986.

Krizay 1989

Krizay J. Partial Hospitalization: Facilities, Cost and Utilization. Washington DC: American Psychiatric Association, Office of Economic Affairs, 1989.

Leucht 2005a

Leucht S, Kane JM, Kissling W, Hamann J, Etschel E, Engel R. Clinical implications of Brief Psychiatric Rating Scale scores. *British Journal of Psychiatry* 2005;**187**:366-71. [PUBMED: 16199797]

Leucht 2005b

Leucht S, Kane JM, Kissling W, Hamann J, Etschel E, Engel RR. What does the PANSS mean?. *Schizophrenia Research* 2005;**79**(2-3):231-8. [PUBMED: 15982856]

Marshall 1999

Marshall M. Psychiatric services: designs to reduce hospital admission. *Clinical Evidence* 1999;**2**:378-84.

Marshall 2000

Marshall M, Lockwood A, Adams C, Bradley C, Joy C, Fenton M. Unpublished rating scales - a major source of bias in randomised controlled trials of treatments for schizophrenia?. *British Journal of Psychiatry* 2000;**276**:249-53.

Marshall 2001

Marshall M, Crowther R, Almaraz-Serrano AM, Tyrer P. Day hospital versus out-patient care for psychiatric disorders. *Cochrane Database of Systematic Reviews* 2001, Issue 3. [DOI: 10.1002/14651858.CD003240]

Moscowitz 1980

Moscowitz IS. The effectiveness of day hospital treatment: a review. *Journal of Community Psychology* 1980;**8**:155-64.

Oliver 1996

Oliver JPJ, Huxley P, Bridges Y, Mohamad H. Quality of Life and Mental Health Services. London, UK: Routledge, 1996.

Overall 1962

Overall JE, Gorham DR. The brief psychiatric rating scale. *Psychological Reports* 1962;**10**:799-812.

Pang 1985

Pang J. Partial hospitalization: an alternative to inpatient care. *Psychiatric Clinics of North America* 1985;**8**:587-95.

Parker 1990

Parker S, Knoll JL. Partial hospitalization: an update. *American Journal of Psychiatry* 1990;**147**:156-60.

Platt 1981

Platt SD, Hirsch SR, Knights AC. Effect of brief hospitalisation on psychiatric patients' behaviour and social functioning. *Acta Psychiatrica Scandinavica* 1981;**63**:117-28.

Priebe 1995

Priebe S, Gruyters T, Heinze M, Hoffmann C, Jäkel A. Subjective criteria for evaluation of psychiatric care: methods for assessment in research and routine care. *Psychiatrische Praxis* 1995;**22**:140-4.

Pryce 1982

Pryce IG. An expanding "stage army" of long-stay psychiatric day-patients. *British Journal of Psychiaty* 1982;**141**:595-601.

RevMan 2008 [Computer program]

The Nordice Cochrane Centre, The Cochrane Collaboration. Review Manager. Version 5.0. 2008: The Nordice Cochrane Centre, The Cochrane Collaboration, 2008.

Rosie 1987

Rosie JS. Partial hospitalization: a review of recent literature. *Hospital and Community Psychiatry* 1987;**38**:1291-9.

Schene 1986

Schene AH, Gersons BPR. Effectiveness and application of partial hospitalization. *Acta Psychiatria Scandinavia* 1986;**74**:335-40.

Schene 1988

Schene AH, Van Lieshout PAH, Mastboom JCM. Different types of partial hospitalization programs: results of a nationwide survey in the Netherlands. *Acta Psychiatrica Scandinavia* 1988;**78**:515-22.

Ukoumunne 1999

Ukoumunne OC, Gulliford MC, Chinn S, Sterne JAC, Burney PGJ. Methods for evaluating area-wide and organisation-based intervention in health and health care: a systematic review. *Health Technology Assessment* 1999;**3**(5):1-75.

Vaughn 1983

Vaughn P. The disordered development of day care in psychiatry. *Health Trends* 1983;**15**:91-4.

Volovik 1986

Volovik VM, Zachepitskii RA. Treatment, care, and rehabilitation of the chronic mentally ill in the USSR. *Hospital and Community Psychiatry* 1986;**37**:280-2.

Weissman 1981

Weismann MM, Sholomskas D, John K. The assessment of social adjustment: an update. *Archives of General Psychiatry* 1981;**38**:1250-8.

Wiersma 1988

Wiersma D, DeJong A, Ormel J. The Groningen social disabilities schedule: Development, relationship with the ICIDH, and psychometric properties. *International Journal of Rehabilitation Research* 1988;**11**:213-24.

Wiersma 1990

Wiersma D, De Jong A, Kraaijkamp HJM, Ormel J. GSDS II: The Groningen Social Disabilities Schedule, Second Revision: Manual, Questionnaire and Rating Form. Second Edition. Groningen, Germany: Department of Social Psychiatry, University of Groningen, 1990.

Wilkinson 1984

Wilkinson G. Day care for patients with psychiatric disorders. *BMJ* 1984;**288**:1710-1.

Wing 1972

Wing JK, Cooper JE, Sartorius N. The Measurement and Classification of Psychiatric Symptoms: An Instruction Manual for the Present State Examination and CATEGO Program. London: Cambridge University Press, 1972.

Xia 2007

Xia J, Adams CE, Bhagat N, Bhagat V, Bhoopathi P, El-Sayeh H, Pinfold V, Takriti Y. The Leeds outcomes stakeholders survey (LOSS) study. Proceedings of the 15th Cochrane Colloquium; 2007 Oct 23-27; Sao Paulo. The Cochrane Collaboration, 2007.

References to other published versions of this review

Marshall 2002

Marshall M, Crowther R, Almaraz-Serrano AM, Sledge WH, Kluiter H, Hill E, Rathbone J, Soares-Weiser K. Day hospital versus admission for acute psychiatric disorders. *Cochrane Database of Systematic Reviews* 2003, Issue 1. [DOI: 10.1002/14651858.CD004026]

* Indicates the major publication for the study

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Creed-UK-1990

Methods	Allocation: randomised, sealed envelope. Blindness: no (evaluation by rater independent of treating clinician, not blind to group allocation). Follow-up: 3, 12 months follow-up. Setting: acute day hospital in inner city. Analysis: intention to treat. Place: Manchester, UK.
Participants	Diagnosis: schizophrenia 23.5%, mood disorder 25.4%, other 51%. N = 102. Age: ~ 42 years. Sex: M 56%, F 44%. History: acutely ill patients requiring hospital admission, not involuntary patient, not too ill for day care and no social factors that made day care impractical.
Interventions	1. Acute day hospital: 8 nurses, 3 OTs (N = 51). 2. Routine inpatient (N = 51).
Outcomes	Lost to follow-up. Readmitted. Hospital service outcomes: duration index admission (estimated from IPD), inpatient & day patient days/month (IPD). Mental state: PSE (IPD). Social functioning: SBAS Role (IPD). Burden on relatives: SBAS Burden (IPD). Costs of hospital care (estimated from IPD). Unable to use - Mental state: Hamilton rating scale (only measured depressive symptoms). Social behaviour: SBAS behaviour (role functioning used as key indicator of social functioning).
Notes	Type 1 trial (IPD obtained). Loss to follow up: 31%.

Risk of bias

Bias	Authors' judgement	Support for judgement

Creed-UK-1990 *(Continued)*

Random sequence generation (selection bias)	Low risk	Randomly assorted cards in sealed envelopes in blocks of six.
Allocation concealment (selection bias)	Low risk	Sealed envelopes.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.
Incomplete outcome data (attrition bias) All outcomes	High risk	Day treatment group: 6 could not be traced, 10 did not attend sufficiently to be assessed, 6 had to be transferred to inpatient care. Inpatient group: 9 could not be traced, 3 discharged themselves before they could be fully assessed. Further analyses considered only those patients who were fully assessed.
Selective reporting (reporting bias)	Low risk	Expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Study was performed with grants from the National Unit for Psychiatric Research and Development and the Department of Health and Social Security.

Creed-UK-1996

Methods	Allocation: randomised, sealed envelope. Blindness: no (evaluation by rater independent of treating clinician, not blind to group allocation). Follow-up: 0.5, 1, 2, 3, 6, 12 months. Setting: acute day hospital in inner-city. Analysis: intention to treat. Place: Manchester, UK.
Participants	Diagnosis: schizophrenia 38.5%, mood disorder 30%, other 31.5%. N = 187. Age: mean ~ 38 years. Sex: M 54.5%, F 45.5%. History: acutely ill patients presenting for admission at the psychiatric day hospital, not involuntary patient, not too ill for day care, not admission for detox and no organic brain disease, personality disorder or mania.
Interventions	1. Acute day hospital CPN out of hours (N = 94). 2. Routine inpatient (N = 93).
Outcomes	Lost to follow-up. Readmitted. Hospital service outcomes: duration index admission (IPD), inpatient & day patient days/month (IPD). Mental state: CPRS (IPD). Social functioning: SBAS Role (IPD). Burden on relatives: SBAS Burden (IPD). Costs of care (IPD). Unable to use - Social behaviour: SBAS behaviour (role functioning used as key indicator of social functioning).

Creed-UK-1996 *(Continued)*

	Burden on relatives: GHQ (this is a measure of depression rather than burden, a more extensive measure of burden from this trial already included (SBAS) - depression in relatives was not an outcome included in this review.
Notes	Type 1 trial (IPD obtained). Loss to follow-up: 23.5%.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Randomly assorted cards in sealed envelopes in blocks of six.
Allocation concealment (selection bias)	Low risk	Sealed envelopes opened by an independent administrator.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.
Incomplete outcome data (attrition bias) All outcomes	High risk	4 inpatient and 4 day patients were excluded due to diagnosis or early discharge. Five inpatients were transferred to the day hospital because of lack of beds, and 11 day patients were transferred to the inpatient unit because they were too ill for the day hospital.
Selective reporting (reporting bias)	Low risk	Expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Study funded by the Department of Health, the North Western Regional Health Authority, and the Mental Health Foundation.

Dick-UK-1985

Methods	Allocation: randomised - no further details. Blindness: no (evaluation by an independent research psychiatrist, not blind to group allocation). Follow up: 0, 3, 12 and 52 weeks. Setting: acute day hospital. Analysis: intention to treat. Place: Dundee, UK.
Participants	Diagnosis: neurosis, personality disorder, or adjustment reaction. N = 91. Age: mean ~ 35 years. Sex: M 32.4%, F 67.6%. History: patients admitted as emergencies with neurosis, personality disorder, or adjustment reaction that were suitable for day hospital treatment (excluded if too ill, suicidal, or day care impractical).
Interventions	1. Acute day hospital: 2 trained staff + OT, patient/staff ratio: 12.5:1, individual counselling, groups, activities and medication (N = 43). 2. Inpatient care: mixed sex and female wards (N = 48).
Outcomes	Lost to follow-up. Readmitted. Satisfaction with care.

Dick-UK-1985 *(Continued)*

	Hospital service outcomes: duration of index admission. Mental state: CIS. Cost of index admission. Unable to use - Continuing medication at one year (not an outcome for this review - unclear whether continuing to take medication at one year is a good or bad outcome in this population).
Notes	Type 1 trial (contacted but IPD no longer exists). Lost to follow up: 29.6%.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Randomised. No further details given.
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	91 patients were enrolled in the study, 64 patients were followed up to one year. Reasons for default were split about equally between the patient having moved to an unknown address, and the patient refusing further co-operation. No further details given.
Selective reporting (reporting bias)	Low risk	Expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Study supported by a grant from the Health Services Research Committee of the Chief Scientist, Scottish Home and Health Department.

Herz-US-1971

Methods	Allocation: randomised by random number table (candidates admitted to inpatient care, then evaluated and those eligible for day hospital randomly allocated). Blindness: no (evaluation by independent research interviewers, not blind to group allocation). Follow up: 0.5, 1, 5, 24 months. Setting: acute day hospital. Analysis: intention to treat. Place: New York State, USA.
Participants	Diagnosis: schizophrenia 36%, other 64%. N = 90. Age: mean ~ 32 years. Sex: M 41%, F 59%. History: not too psychiatrically ill for day care, not too psychiatrically healthy for inpatient care.
Interventions	1. Acute day hospital: 5 weekdays attendance, 8-4.30pm, group-oriented psychotherapy, patient/staff ratio not reported (N = 45).

Herz-US-1971 *(Continued)*

	2. Routine inpatient care: staff, setting and activities same for both groups (N = 45).
Outcomes	Lost to follow-up. Deaths. Readmitted. Hospital service outcomes: duration of index admission. Unable to use - Mental state: Psychiatric Evaluation Form, Psychiatric Status Schedule (no summary data).
Notes	Type 1 trial (contacted, but IPD no longer exists). Lost to follow up: 18.8%.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Table of random numbers.
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.
Incomplete outcome data (attrition bias) All outcomes	High risk	At each follow-up period, the number of patients actually evaluated was fewer than the 45 who were in each group ("The two- and four-week cross-section evaluations were not done on the first 13 patients"; some could not be interviewed: "out of town"; "could not be located"; "refused to be interviewed"; "patient no longer in therapy with [the resident]"). No further details given.
Selective reporting (reporting bias)	High risk	Not all expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Source of funding not reported.

Kallert-EU-2007

Methods	Allocation: randomised, opaque, sealed envelopes. Blindness: no (evaluation by researchers independent of treating clinicians, not blind to group allocation). Follow up: 0, 3, 12 months. Setting: day hospitals in 5 centres. Analysis: intention to treat. Place: Dresden, Germany; London, UK; Wroclaw, Poland; Michalovce, Slovak Republic; and Prague, Czech Republic.
Participants	Diagnosis: schizophrenia 26%, mood disorder 33%, other 33%. N = 1117. Age: mean ˜ 38 years. Sex: M 44%, F 56%.

Kallert-EU-2007 *(Continued)*

	History: presented with a mental disorder that had disturbed at least 1 area of daily living or jeopardised the residential, financial or occupational status of the patient or their family, other treatments inadequate.
Interventions	1. Acute day hospital: provided between 15 and 35 places, mean staff hours per week per treatment place ranged from 8.8 to 16.0. Staff patient ratios not reported (N = 596). 2. Routine inpatient care (N = 521).
Outcomes	Lost to follow-up. Mean duration of admission. Mental state: BPRS. Social functioning: GSDs-II. Treatment satisfaction: CAT. Quality of life: MANSA.
Notes	Type 1 trial. Loss to follow up: 31.9%. 3 suicides occurred in the inpatient group, it is assumed that these represent all deaths in this study. Funding: NHS Executive. The study has joined a European multi-centred project evaluating similar services in Prague (Czech Republic), Dresden (Germany), Wroclaw (Poland), and Michalovce (Slovakia).

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	“A computerized random-number generator created an allocation sequence”.
Allocation concealment (selection bias)	Low risk	“Opaque, sealed envelopes”.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blinded.
Incomplete outcome data (attrition bias) All outcomes	Low risk	“The initial attrition rates from randomization to admission varied significantly among settings and centres, with rates for the total sample of 7.9% for those allocated to day hospitals and 1.5% for those allocated to day hospitals”; “follow-up rates for the total sample assessed at admission were 87.0% at discharge, 76.5% 3 months after discharge, and 68.1% 12 months after discharge”. Missing values were imputed.
Selective reporting (reporting bias)	Low risk	Expected outcomes reported.
Other bias	Low risk	Study supported by the European Commision (Quality of life and Management of Living Resources Programme: QLG4-CT-2000-01700). Support for travel and accommodation for EDEN project meetings provided by Pfizer Pharmaceutical Co.

Kris-US-1965

Methods	Allocation: randomised at time of relapse. Blindness: no. Follow up: 2 months after discharge.

Kris-US-1965 *(Continued)*

	Setting: acute day hospital. Analysis: intention to treat. Place: New York, USA.
Participants	Diagnosis: not reported, but all had suffered from "psychosis". Inclusion criteria: previously treated in hospital for psychotic symptoms. N = 141. Age: mean unknown. Sex: F unknown, M unknown. History: ethnic minority % unknown, married % unknown, unemployed % unknown, mean previous admissions % unknown.
Interventions	1. Acute day hospital: weekdays, 9-5pm, patient/staff ratio not reported, emphasis on milieu & group therapy (N = 71). 2. Standard inpatient treatment (N = 70).
Outcomes	Employed. Unable to use - Hospital service outcomes: days in hospital (mean, SD not reported). Mental state: Wittenborn rating scale (no data reported).
Notes	Type 1 trial (unable to contact). Lost to follow up: not clear.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Randomly selected, no further details given.
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	No information provided.
Selective reporting (reporting bias)	High risk	Not all expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Source of support not reported.

Schene-NL-1993

Methods	Allocation: randomised - no further details, but 14 later withdrawn because of "incorrect randomisation". Follow up: at 6 months following discharge. Evaluation: unclear if raters independent of treating clinicians, not blind. Analysis: not intention to treat, see notes.

Schene-NL-1993 *(Continued)*

	Lost to follow up: not clear given exclusions. Setting: Acute day hospital at the University of Utrecht, Netherlands.
Participants	Diagnosis: precise estimate not possible because of post-randomisation exclusions. N = 222. Age and sex: uncertain given the exclusions post-randomisation. History: referred for inpatient treatment, no organic brain disease, no primary diagnosis or substance abuse or mental retardation, no other contraindications to day treatment.
Interventions	1. Acute day hospital: staff patient ratio 1:12.5, emphasis on psychosocial therapy (N = 99). 2. Standard inpatient care: University psychiatric clinic (N = 123).
Outcomes	Lost to follow-up. Unable to use - Hospital Service Outcomes: days in hospital (not an intention-to-treat analysis). Mental state: PSE, SCL-90 (not an intention-to-treat analysis). Social Functioning: Groningen Social Disabilities Schedule, Social Network and Social Support Questionaire (not an intention-to-treat analysis).
Notes	Type 1 trial (no attempt to obtain IPD as not an intention-to-treat analysis). Lost to follow-up: 32%. Not an intention-to-treat analysis as 72 patients were excluded after randomisation including any day patients transferred to a closed ward for more than 28 days.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	"Randomised". No further details given but 14 later withdrawn because of "incorrect randomisation procedure".
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.
Incomplete outcome data (attrition bias) All outcomes	High risk	On admission: "21 (9%) of all randomized patients had to be transferred to a closed ward"; "10 (5%) patients did not accept the result of the randomization" "28 (13%) patients decided [...] against admission" "4 patients (2%) refused to participate in the study" "9 (4%) were excluded for other reasons". 31 (21%) patients had dropped out by discharge, 12 (8%) patients dropped out at 6 month follow up ("admission less than 28 days"; "transfer to a closed ward for more than 28 days"; "patients' refusal to participate").
Selective reporting (reporting bias)	High risk	Not all expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Supported by grants from the Prevention Fund and National Fund for Mental Health (Netherlands).

Sledge-US-1996

Methods	Allocation: computer-generated randomisation by a researcher unaware of patient characteristics - however, if no bed available candidate was allocated to the other condition. Blindness: no (evaluation by rater independent of treating clinician, but not blind to group allocation). Follow up: discharge, 2, 5, 10 months. Setting: day hospital of a community mental health centre day hospital. Analysis: intention to treat. Place: New Haven, Connecticut, USA.
Participants	Diagnosis: schizophrenia 39%, mood disorder 52%, other 9%. N = 197. Age: mean ~ 33 years. Sex: M 51%, F 49%. History: presenting for inpatient admission, living locally, not involuntary, not too ill for day patient treatment, not intoxicated or medically unwell.
Interventions	1. Acute day hospital: crisis respite programme + 'back up' bed if necessary, day hospital = 20 patient facility with doctors, nurses, social workers, therapists, weekdays 9-3pm, group work, control of symptoms & improvement of daily skills (N = 93). 2. Inpatient care: 36-bed unit with doctors & nursing staff, psychologist, mental health workers + very active programme (N = 104).
Outcomes	Lost to follow-up. Readmitted. Hospital service outcomes: duration of index admission (IPD), inpatient & day patient days/month (IPD). Mental state: BPRS. Social functioning: SAS. Costs of care. Unable to use - Global functioning: GAS (not an outcome in this review). Mental state: SCL-90 (redundant measurement - BPRS also used).* Quality of life: Connecticut Department of Health Quality of Life Survey (unpublished scale). Satisfaction: Satisfaction with Services Scale (unpublished scale).
Notes	Type 1 trial (IPD obtained). Lost to follow up: 28.4%. * Our IPD analysis required us to choose between the two measure of mental state (BPRS or SCL 90) used in this study - BPRS was chosen because it was more similar to the CPRS used in the two Creed studies - the two scales have similar effect sizes in Sledge-US-1996.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	"Random assignment"; "if a consenting patient was randomly assigned to a treatment setting that was full [...] the patient was assigned (i.e. “rolled over” or switched) to the other condition."
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.
Incomplete outcome data (attrition bias)	Unclear risk	"Attrition from the panel of 197 patients who completed the initial interview was 7% (N = 14) at the discharge interview, 25% (N = 49) at the 2-month fol-

Sledge-US-1996 *(Continued)*

All outcomes		low-up, 25% (N = 49) at the 5-month follow-up, and 28% (N = 55) at the 10-month follow-up".
Selective reporting (reporting bias)	Low risk	Expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Supported by grant SMH-47638 from the Substance Abuse and Mental Health Source Administration (USA).

Wiersma-NL-1989

Methods	Allocation: randomisation by block. Blindness: no (evaluation by independent raters who were not blind to group allocation). Follow-up: 1 and 2 years. Setting: acute day hospital operated by the Regional Institute for Ambulatory Mental Health Care. Analysis: intention to treat. Place: Groningen, Netherlands.
Participants	Diagnosis: schizophrenia 33.1%, mood disorder 30.1%, other 36.8%. N = 160. Age: mean ~ 42 years. Sex: M 50%, F 50%. History: presenting for admission, forensic patients on court order and patients with dementia.
Interventions	1. Acute day hospital: admitted as soon as considered no risk to self or others, weekdays 8.30-16.30, could be inpatient for 1-2 nights on demand, 24 hr on call line to nurse (N = 103). 2. Routine inpatient (N = 57).
Outcomes	Lost to follow-up. Deaths. Readmitted. Unemployed. Hospital service outcomes: days in hospital care (IPD). Mental state: PSE (IPD). Social functioning: Groningen Social Disability Scale (IPD).
Notes	Type 2 trial (IPD obtained). Lost to follow up: 41% at 2 years.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	"Randomised in blocks". No further details given.
Allocation concealment (selection bias)	Unclear risk	No information provided.
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.

Wiersma-NL-1989 *(Continued)*

Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Only 34 of the 54 schizophrenic patients (68%) participated in the 2 year interviews, 24 (71%) of 34 experimental and 10 (63%) of 16 controls. No further details given.
Selective reporting (reporting bias)	High risk	Not all expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Source of support not reported.

Zwerling-US-1964

Methods	Allocation: randomisation via list held by phone answering service (fixed ratio). Follow up: 2 years. Evaluation: by rater independent of treating clinician, but not blind to group allocation. Setting: acute day hospital. Analysis: not an intention to treat analysis, patients with organic brain disease were randomised but then excluded. Coutry: New York, USA.
Participants	Diagnosis: not reported. N = 378. Age: not reported. Sex: not reported. History: people about to be admitted were allocated to day hospital or inpatient treatment.
Interventions	1. Acute day hospital: group oriented activities + family therapy, reviewed twice weekly, weekdays (N = 189). 2. Routine inpatient care (N = 189).
Outcomes	Unable to use - Leaving the study early (8% lost, but proportion from each group not reported). Deaths (not an intention-to-treat analysis, people with organic brain disease were excluded from the study after randomisation). Readmitted (not an intention-to-treat analysis, people with organic brain disease were excluded from the study after randomisation).
Notes	Type 2 trial (unable to contact). Lost to follow-up: 8%.

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Random list of "D" (day hospital) and "I" (inpatient) prepared and numbered sequentially. Day hospital project book contained the number sequence. Each patient entered into the book in sequence.
Allocation concealment (selection bias)	Low risk	Telephone answering service revealed name and number of patient, and then the random designation of "D" or "I".
Blinding (performance bias and detection bias) All outcomes	High risk	Not blind.

Zwerling-US-1964 *(Continued)*

Incomplete outcome data (attrition bias) All outcomes	High risk	Reasons for rejection of patients designated to day hospitalisation: 22 (34%) had medical or surgical problems, 2 patients did not have family to provide medical care at home, 9 (14%) had travel complications, 20 (31%) patients behaviour required 24-hour hospitalisation. In 8 cases, patients remained in inpatient care after being admitted during the night or weekend.
Selective reporting (reporting bias)	High risk	Not all expected outcomes reported.
Other bias	Unclear risk	No sample size calculation. Protocol not available. Supported by a grant from the National Institute of Mental Health (MH-01132).

General abbreviations
~ - approximately
CPN - Community Psychiatric Nurse
IPD - individual patient data
OT - Occupational therapist
Scales
BPRS - Brief Psychological Rating Scale
CIS - Clinical Interview Schedule
CPRS - Comprehensive Psychopathological Rating Scale
GAS - Global Assessment Scale
GHQ - General Health Questionnaire
PSE - Present State Examination
SCL 90 - Symptom Check List
SAS - Social Adjustment Scale
SBAS - Social Behaviour Assessment Schedule

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Austin-Los Angeles	Allocation: not randomised, survey comparing randomly selected people from two different day hospitals.
Azim-Alberta	Allocation: not randomised, quasi-experimental design, comparing inpatients, day hospital patients and non-patient controls.
Barkley-Ontario	Allocation: not randomised, retrospective study.
Basker-Jerusalem	Allocation: not randomised, before and after design.
Bateman-London	Allocation: randomised. Participants: people with personality disorders. Intervention: care in a psychotherapeutic day hospital versus outpatient care, not acute day hospital care versus admission.
Beigel-New York	Allocation: not randomised, quasi-experimental design, comparing people who completed a partial hospitalisation program with those who dropped out.
Bertrand-Belgium	Allocation: not randomised.
Boath-Stoke	Allocation: not randomised, quasi-experimental design comparing people in a day treatment program for post-natal depression with controls in primary care.

Study	Reason for exclusion
Bowman-Dublin	Allocation: not randomised, survey examining differences between people admitted day hospital and inpatient care.
Bradshaw-Minnesota	Allocation: randomised. Participants: people with schizophrenia who were long term attenders at a day care centre. Intervention: day care + cognitive behavioural therapy versus day care alone, not acute day hospital care versus admission.
Brook-Denver	Allocation: not randomised, survey comparing people treated in a crisis hostel with those treated in inpatient care.
Carey-US	Allocation: randomised. Participants: attenders at a day care centre who also abused substances. Intervention: problem-solving training + day care versus day care alone, not acute day hospital care versus admission.
Case-New York	Allocation: not randomised, retrospective study.
Comstock-Texas	Allocation: not randomised, retrospective multivariate analysis.
Creed-Blackburn	Allocation: randomised by sealed envelope, however, the trialists judged that the randomisation procedure had been compromised as people allocated to the day hospital condition were much less disabled that those admitted to inpatient care (available data bear this out in terms of diagnosis and behaviour).
Creed-Manchester	Allocation: not randomised, quasi-experimental study comparing consecutive admission to day hospital and inpatient care.
Dal Santo 2004	Allocation: not randomised.
Davidson 2006	Allocation: randomised. Intervention: CBT versus TAU, not day hospital versus inpatient care.
Dick-Dundee	Allocation: randomised. Participants: patients with chronic anxiety and depression. Intervention: day hospital versus continuing outpatient care, not acute day hospital care versus admission.
Drake-New Hampshire	Allocation: not randomised, quasi-experimental design, comparing day treatment with supported employment program.
Ettlinger-New York	Allocation: not randomised, case-control study of day hospital versus inpatient care.
Fink-Toronto	Allocation: not randomised, quasi-experimental study of inpatient care versus day patient care.
Glick-New York	Allocation: randomised (method not clear). Participants: people with severe mental illness recently discharged from hospital. Interventions: transitional day hospital programme versus out patient follow-up, not acute day hospital care versus admission.
Glick-San Francisco	Allocation: randomised. Participants: people requiring hospital inpatient care. Intervention: short versus long hospital admission, not acute day hospital care versus admission.
Grad-Chichester	Allocation: not randomised, quasi-experimental design comparing community care in two towns.

Study	Reason for exclusion
Gudeman-Boston	Allocation: not randomised, before and after design.
Guidry-New Orleans	Allocation: not randomised, before and after design.
Guillette-Maryland	Allocation: not randomised, survey comparing costs of day patient care with theoretical costs of in-patient care.
Guy-Baltimore	Allocation: randomised by sealed envelope. Participants: people with a variety of psychiatric disorders referred for day care. Intervention: day hospital treatment versus outpatient care, not acute day hospital care versus admission.
Herz-New York2	Allocation: randomised (method not specified). Participants: people with acute psychiatric disorders about to be admitted to inpatient care. Interventions: routine inpatient care versus brief inpatient care versus brief inpatient plus day care, not acute day hospital care versus admission.
Hirsch-London	Allocation: random allocation (method not specified). Participants: people with acute psychiatric disorders about to be admitted to inpatient care. Interventions: brief inpatient care with some use of day hospital (47% patients in the brief care group were exposed to day hospital) versus routine inpatient care, not acute day hospital care versus admission.
Hogg-Glasgow	Allocation: not randomised, a survey comparing long term inpatients with long term day patients.
Inch-Saskatchewan	Allocation: not randomised, a prospective study comparing day hospital patients receiving 'therapeutic' and 'non-therapeutic' discharges.
Jarema-Warsaw	Allocation: not randomised, a survey comparing quality of life scores between day hospital patients, inpatients and outpatients.
Kandel-US	Allocation: randomised. Participants: adult general psychiatry patients attending a day treatment program. Intervention: day treatment plus a small group intervention compared against day treatment, in order to assess effect on "future time perception", not acute day hospital care versus admission.
Kecmanovic-Sarajevo	Allocation: not randomised, case-control study comparing discharged inpatients with discharged day patients.
Klyczek-US	Allocation: not randomised, quasi-experimental design comparing outcome in two day hospitals, one of which offered mainly psychotherapy, whilst the other offered mainly activity therapy.
Konieczynska-Warsaw	Allocation: not randomised, follow-up study comparing the outcome for patients treated in a day hospital, inpatient ward and community mental health team.
Kuldau-California	Allocation: randomised. Participants: inpatients about to be discharged. Interventions: rapid discharge from inpatient care versus community transitional system (34%subjects of intervention group were discharged via day hospital), not acute day hospital care versus admission.
Levenson-Houston	Allocation: randomised by table of random numbers. Participants: people with acute schizophrenia. Intervention: treatment in an outpatient clinic versus hospital admission, excluded as outpatient clinic does not meet criteria for day hospital.

Study	Reason for exclusion
Liang-Taipei	Allocation: not randomised, a survey comparing quality of life in patients in various care settings, including day hospitals.
Linn-USA	Allocation: randomised by sealed envelope. Participants: people with schizophrenia about to be discharged from hospital. Interventions: day hospital treatment or outpatient care, not acute day hospital care versus admission.
Lystad-Louisiana	Allocation: not randomised, quasi-experimental design.
Mathai-Bangalore	Allocation: not randomised, survey.
McDonnell-Ireland	Allocation: not randomised, case report of a day hospital care in Dublin, Ireland.
Meltzoff-New York	Allocation: randomised by sealed envelope. Participants: people with a variety of mental disorders referred for day care. Interventions: day hospital treatment versus outpatient care, not acute day hospital care versus admission.
Michaux 1969	Allocation: not randomised.
Michaux-Maryland	Allocation: not randomised, quasi-experimental study of inpatient care versus day hospital care.
Milne-Wakefield	Allocation: not randomised, quasi-experimental study.
Newton-US	Allocation: inadequate randomisation procedure, participants assigned alternatively to inpatient (even numbered) or day hospital (odd numbered).
Niskanen-Helsinki	Allocation: not randomised, compared patients before and after treatment in a day hospital.
O'Shea-Ireland	Allocation: not randomised, retrospective cost-effectiveness analysis comparing day patients and inpatients.
Odenheimer-USA	Allocation: not randomised, survey of the relatives of day hospital patients.
Oka-Kurume-Japan	Allocation: not randomised, quasi-experimental design comparing outcome in 31 patients with schizophrenia entering a day care centre with that of 30 outpatients with schizophrenia matched for age and sex.
Pang-US	Allocation: not randomised, narrative review.
Penk-Dallas	Allocation: not randomised, case-control study of day hospital versus inpatient care.
Piersma-Michigan	Allocation: not randomised, quasi-experimental study compared improvement in a group of inpatients with that in a group in day hospital.
Piper-Alberta	Allocation: randomised. Participants: outpatients with affective and personality disorders. Intervention: outpatient treatment of day hospital care, not acute day hospital care versus admission.
Platt-London	Allocation: randomised. Participants: people with acute psychiatric disorders Intervention: admission to day hospital versus inpatient care, trial abandoned when insufficient people (10) were randomised in first 10 weeks. No data available.

Study	Reason for exclusion
Prior-Middlesex	Allocation: not randomised.
Russell-Ottawa	Allocation: not randomised, outcome for day patients compared with a retrospectively obtained sample of inpatients.
Sandell-Stockholm	Allocation: not randomised, cohort study.
Shek 2009	Allocation: not randomised, systematic review. Participants: acutely ill.
Skoda-Czech Republic	Allocation: randomised. Participants: people with neurosis, not schizophrenia.
Tam-Hong Kong	Allocation: not randomised, survey comparing day patients with inpatients on demographic and psychological variables.
Tantam-Manchester	Allocation: not randomised, case-control study of a rehabilitation treatment for long-stay day patients.
Tsukahara 1998	Allocation: not randomised.
Tyrer-Southampton	Allocation: randomised by sealed envelope. Participants: people with depression and anxiety. Interventions: outpatient treatment versus two varieties of day care, not acute day hospital care versus admission.
Vaglum-Oslo	Allocation: not randomised, follow-up study comparing outcome in day patients with different types of personality disorder.
Vaitl-Haar-Germany	Allocation: not randomised, retrospective study comparing outcome in patients treated at day hospitals with those treated at "night" hospitals.
Van Den Hout-NL	Allocation: randomised. Participants: depressed patients on a day treatment program. Intervention: self-control therapy plus day care versus day care, not acute day hospital care versus admission.
Washburn-Boston	Allocation: randomised, method not specified. Participants: women receiving inpatient treatment. Intervention: continuing inpatient admission versus discharge to day patient care, not acute day hospital care versus admission.
Weissert 1980	Allocation: randomised. Participants: chronically ill, no mention of acute psychiatric disorders.
Welburn-Ottawa	Allocation: not randomised, quasi-experimental design in which outcome for patients participating in a psychotherapy-oriented day treatment program was compared against outcome for those awaiting admission to the program.
Weldon-New York	Allocation: randomised, method not specified. Participants: people about to be discharged from inpatient care. Intervention: day hospital treatment versus outpatient care, not acute day hospital care versus admission.
Wilberg-Oslo	Allocation: not randomised, quasi-experimental study of day treatment + psychotherapy versus day treatment alone, for people with borderline personality disorder.

Study	Reason for exclusion
Wu 1995	Allocation: randomised. Intervention: sulpiride vs olanzapine vs sulpiride + olanzapine. (translated with support from Cochrane Schizophrenia Group).

Characteristics of studies awaiting assessment *[ordered by study ID]*

Vietze-Germany

Methods	
Participants	
Interventions	
Outcomes	
Notes	Reference in German, awaiting translation.

Characteristics of ongoing studies *[ordered by study ID]*

Donnison 2001

Trial name or title	An evaluation of the clinical effectiveness of cognitive-behavioural rapid stabilisation group therapy in a day hospital setting.
Methods	Allocation: randomised.
Participants	History: people considered suitable for day hospital admission. Exclusion criteria: active psychosis; in the manic phase of bipolar disorder; substance misuse which comprises a person's ability to participate.
Interventions	1. Cognitive behavioural group therapy: aimed at rapid stabilisation in combination with treatment as usual (TAU). 2. TAU alone.
Outcomes	Mental state: Brief Symptom Inventory (BSI), Beck Depression Inventory (BDI), Beck Hopelessness Scale (BHS), Hospital Anxiety and Depression Scale. General state: The General Health Questionnaire (GHQ). Social functioning: Social Functioning Scale (SFS). Cognition: Cognitive Skills Survey. Satisfaction: a client satisfaction measure.
Starting date	1 April 2001
Contact information	Ms Jenny Donnison Community Health Sheffield NHS Trust Eastglade Centre 1 EastGlade Crescent Sheffield S12 4QN UK Telephone: (0114) 271 6454

Donnison 2001 *(Continued)*

	Fax: (0114) 271 6450
Notes	

Gjonbalaj-Marovic 2005

Trial name or title	Brief Community Linkage Intervention for Dually Diagnosed Individuals.
Methods	Allocation: randomised.
Participants	History: inclusion criteria - patients over 18 years old; have a substance abuse disorder + diagnosis of schizophrenia, schizoaffective disorder, or bipolar I disorder; seeking outpatient treatment for the above disorders from the VA; physically mobile, agree to take public transportation if they do not have other private sources. Exclusion criteria - patients who only have either a mental health problem, or a substance abuse problem, but not both; who do not have a residence where they can stay upon discharge from hospital; who are not sufficiently medically or psychiatrically stable to participate in residential or outpatient treatment; could be re-evaluated for study once stabilised; exclusively engaged in methadone maintenance programs; who represent a serious suicide risk.
Interventions	1. Time limited case management. 2. Health education.
Outcomes	Service use: show rate at outpatient day treatment centre, day treatment attended, days re-hospitalised Completion. Global state: Global Level of Functioning, alcohol use, illicit drug use.
Starting date	June 2005
Contact information	Selvija Gjonbalaj-Marovic (973) 676-1000 selvija.gjonbalajmarovic@va.gov
Notes	

DATA AND ANALYSES

Comparison 1. Day patient verus inpatient care for Type 1 studies

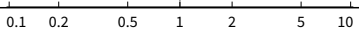
Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Feasibility and engagement: lost to follow-up (at end of study)	7		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
1.1 by 3 months	1	1117	Risk Ratio (M-H, Fixed, 95% CI)	0.97 [0.80, 1.17]
1.2 by 6 months	2	312	Risk Ratio (M-H, Fixed, 95% CI)	0.83 [0.58, 1.19]

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1.3 by about 1 year	5	1694	Risk Ratio (M-H, Fixed, 95% CI)	0.94 [0.82, 1.08]
2 Extent of hospital care: 1a. duration of index admission	4	1582	Mean Difference (IV, Random, 95% CI)	27.47 [3.96, 50.98]
3 Extent of hospital care: 1b. duration of index admission (Type 1 additional data)			Other data	No numeric data
4 Extent of hospital care: 2. duration of all hospital care (days/month)	3	465	Mean Difference (IV, Fixed, 95% CI)	-0.38 [-1.32, 0.55]
5 Extent of hospital care: 3. duration of day patient care (adjusted days/month)	3	465	Mean Difference (IV, Fixed, 95% CI)	2.34 [1.97, 2.70]
6 Extent of hospital care: 4. duration of stay in hospital (days/month)	3	465	Mean Difference (IV, Fixed, 95% CI)	-2.75 [-3.63, -1.87]
7 Extent of hospital care: 5. readmitted to in/day patient care after discharge	5	667	Risk Ratio (M-H, Fixed, 95% CI)	0.91 [0.72, 1.15]
8 Mental state: average end-point score (BPRS, high = poor)	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
8.1 at admission	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.08 [-0.13, -0.03]
8.2 at discharge	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.01 [-0.07, 0.05]
8.3 at 3 months	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.05 [-0.11, 0.01]
8.4 at 12 months	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.05 [-0.11, 0.01]
9 Social functioning: average overall role score (GSDS-II, high = poor)	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
9.1 at admission	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.13 [-0.20, -0.06]
9.2 at discharge	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.34 [-0.48, -0.20]
9.3 at 3 months	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.10 [-0.19, -0.01]
9.4 at 12 months	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.11 [-0.19, -0.03]
10 Burden: average carers' score (SBAS, high = poor)	2		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
10.1 at 14 days	1	85	Mean Difference (IV, Fixed, 95% CI)	0.27 [-2.58, 3.12]

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
10.2 at 1 month	1	95	Mean Difference (IV, Fixed, 95% CI)	-0.04 [-2.54, 2.46]
10.3 at 2 months	1	95	Mean Difference (IV, Fixed, 95% CI)	0.65 [-1.33, 2.63]
10.4 at 3 months	2	160	Mean Difference (IV, Fixed, 95% CI)	-0.59 [-1.62, 0.44]
10.5 at 12 months	1	65	Mean Difference (IV, Fixed, 95% CI)	-0.69 [-2.14, 0.76]
11 Death (all causes)	2	1207	Risk Ratio (M-H, Fixed, 95% CI)	0.18 [0.02, 1.54]
11.1 all-cause death	1	90	Risk Ratio (M-H, Fixed, 95% CI)	0.33 [0.01, 7.97]
11.2 deaths (suicide and untoward events)	1	1117	Risk Ratio (M-H, Fixed, 95% CI)	0.12 [0.01, 2.41]
12 Unemployed (at end of study)	2	320	Risk Ratio (M-H, Fixed, 95% CI)	0.81 [0.67, 0.97]
13 Satisfaction with care: 1. not satisfied with care received	1	91	Risk Ratio (M-H, Fixed, 95% CI)	0.46 [0.27, 0.79]
14 Satisfaction with care: 2. average overall score (CAT, low = poor)	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
14.1 at admission	1	1117	Mean Difference (IV, Fixed, 95% CI)	0.22 [-0.04, 0.48]
14.2 at discharge	1	1117	Mean Difference (IV, Fixed, 95% CI)	0.06 [-0.18, 0.30]
15 Costs of care: 1. raw data			Other data	No numeric data
16 Costs of care: 2. percent differences in costs			Other data	No numeric data
17 Quality of life: average overall role score (MANSA, low = poor)	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
17.1 at admission	1	1117	Mean Difference (IV, Fixed, 95% CI)	-0.02 [-0.13, 0.09]
17.2 at discharge	1	1117	Mean Difference (IV, Fixed, 95% CI)	0.01 [-0.12, 0.14]
17.3 at 3 months	1	1117	Mean Difference (IV, Fixed, 95% CI)	0.11 [-0.02, 0.24]
17.4 at 12 months	1	1117	Mean Difference (IV, Fixed, 95% CI)	0.01 [-0.13, 0.15]

Analysis 1.1. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 1 Feasibility and engagement: lost to follow-up (at end of study).

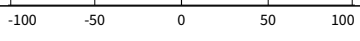
Study or subgroup	Day patients n/N	Hospital admission n/N	Risk Ratio M-H, Fixed, 95% CI	Weight	Risk Ratio M-H, Fixed, 95% CI
1.1.1 by 3 months					
Kallert-EU-2007	163/596	147/521		100%	0.97[0.8,1.17]
Subtotal (95% CI)	**596**	**521**		**100%**	**0.97[0.8,1.17]**
Total events: 163 (Day patients), 147 (Hospital admission)					
Heterogeneity: Not applicable					
Test for overall effect: Z=0.32(P=0.75)					
1.1.2 by 6 months					
Herz-US-1971	11/45	7/45		14.58%	1.57[0.67,3.69]
Schene-NL-1993	26/99	46/123		85.42%	0.7[0.47,1.05]
Subtotal (95% CI)	**144**	**168**		**100%**	**0.83[0.58,1.19]**
Total events: 37 (Day patients), 53 (Hospital admission)					
Heterogeneity: Tau^2=0; Chi^2=2.82, df=1(P=0.09); I^2=64.49%					
Test for overall effect: Z=1.02(P=0.31)					
1.1.3 by about 1 year					
Dick-UK-1985	5/43	3/48		1.02%	1.86[0.47,7.33]
Creed-UK-1990	19/51	13/51		4.68%	1.46[0.81,2.63]
Creed-UK-1996	23/94	23/93		8.33%	0.99[0.6,1.63]
Sledge-US-1996	19/93	37/104		12.58%	0.57[0.36,0.93]
Kallert-EU-2007	208/596	191/521		73.39%	0.95[0.81,1.11]
Subtotal (95% CI)	**877**	**817**		**100%**	**0.94[0.82,1.08]**
Total events: 274 (Day patients), 267 (Hospital admission)					
Heterogeneity: Tau^2=0; Chi^2=7.27, df=4(P=0.12); I^2=44.97%					
Test for overall effect: Z=0.87(P=0.39)					



Favours day patients — Favours inpatients

Analysis 1.2. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 2 Extent of hospital care: 1a. duration of index admission.

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Random, 95% CI	Weight	Mean Difference Random, 95% CI
Creed-UK-1990	41	101.6 (82.8)	48	46.1 (62.9)		19.54%	55.5[24.53,86.47]
Creed-UK-1996	90	91.6 (78.6)	89	55.8 (58.2)		24.24%	35.8[15.55,56.05]
Kallert-EU-2007	596	78 (73)	521	46 (46)		28.79%	32[24.93,39.07]
Sledge-US-1996	93	31.8 (44)	104	36.4 (41.8)		27.44%	-4.6[-16.62,7.42]
Total ***	**820**		**762**			**100%**	**27.47[3.96,50.98]**
Heterogeneity: Tau^2=486.73; Chi^2=31.87, df=3(P<0.0001); I^2=90.59%							
Test for overall effect: Z=2.29(P=0.02)							



Favours day patients — Favours inpatients

Analysis 1.3. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 3 Extent of hospital care: 1b. duration of index admission (Type 1 additional data).

Extent of hospital care: 1b. duration of index admission (Type 1 additional data)

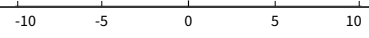
Study	Duration day patient	Duration in patient	Notes
Dick-UK-1985	median 34 days	median 20 days	after adjustment
Herz-US-1971	mean 48.5 days	mean 138.8 days	no statistical test reported

Analysis 1.4. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 4 Extent of hospital care: 2. duration of all hospital care (days/month).

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
Creed-UK-1990	41	5.8 (4.7)	48	5.4 (6)		17.87%	0.39[-1.82,2.6]
Creed-UK-1996	90	4.3 (5)	89	5.4 (5.3)		38.46%	-1.11[-2.61,0.39]
Sledge-US-1996	93	5.1 (5)	104	5.1 (5.1)		43.67%	-0.06[-1.47,1.35]
Total ***	**224**		**241**			**100%**	**-0.38[-1.32,0.55]**

Heterogeneity: Tau^2=0; Chi^2=1.57, df=2(P=0.46); I^2=0%

Test for overall effect: Z=0.81(P=0.42)



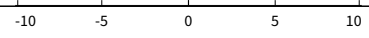
Favours day patients — Favours inpatients

Analysis 1.5. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 5 Extent of hospital care: 3. duration of day patient care (adjusted days/month).

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
Creed-UK-1990	41	4.4 (3.8)	48	0.8 (2.1)		7.72%	3.6[2.28,4.92]
Creed-UK-1996	90	3.5 (3.1)	89	0.7 (1.8)		24%	2.72[1.97,3.47]
Sledge-US-1996	93	2.9 (1.7)	104	0.8 (1.5)		68.28%	2.06[1.62,2.5]
Total ***	**224**		**241**			**100%**	**2.34[1.97,2.7]**

Heterogeneity: Tau^2=0; Chi^2=6.06, df=2(P=0.05); I^2=67.01%

Test for overall effect: Z=12.54(P<0.0001)



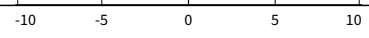
Favours day patients — Favours inpatients

Analysis 1.6. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 6 Extent of hospital care: 4. duration of stay in hospital (days/month).

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
Creed-UK-1990	41	1.4 (3.8)	48	4.7 (5.8)		19.02%	-3.23[-5.24,-1.22]
Creed-UK-1996	90	1.7 (4.4)	89	4.9 (5.2)		38.59%	-3.21[-4.62,-1.8]
Sledge-US-1996	93	2.2 (4.9)	104	4.4 (4.8)		42.39%	-2.11[-3.46,-0.76]
Total ***	**224**		**241**			**100%**	**-2.75[-3.63,-1.87]**

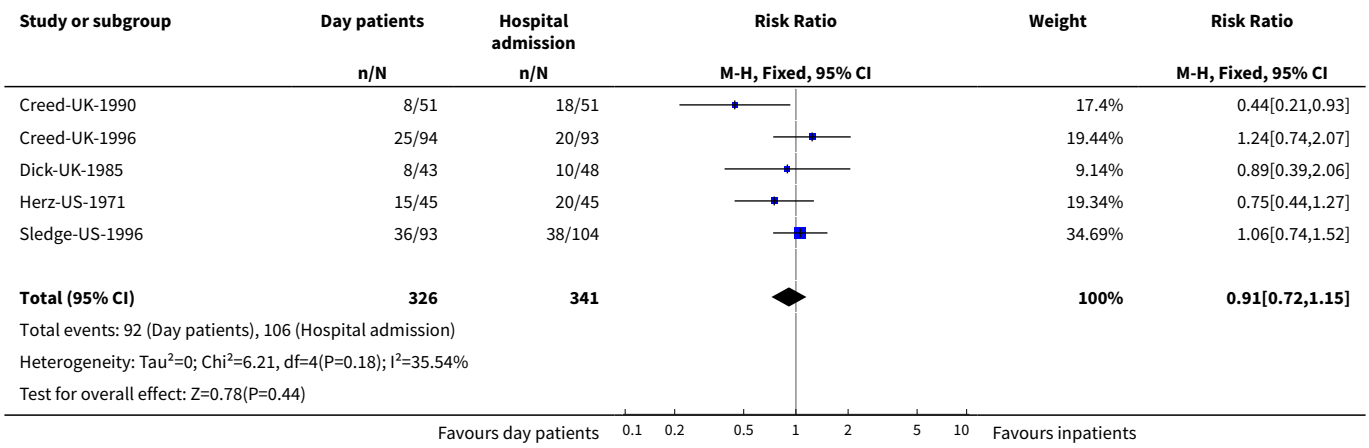
Heterogeneity: Tau^2=0; Chi^2=1.49, df=2(P=0.48); I^2=0%

Test for overall effect: Z=6.13(P<0.0001)

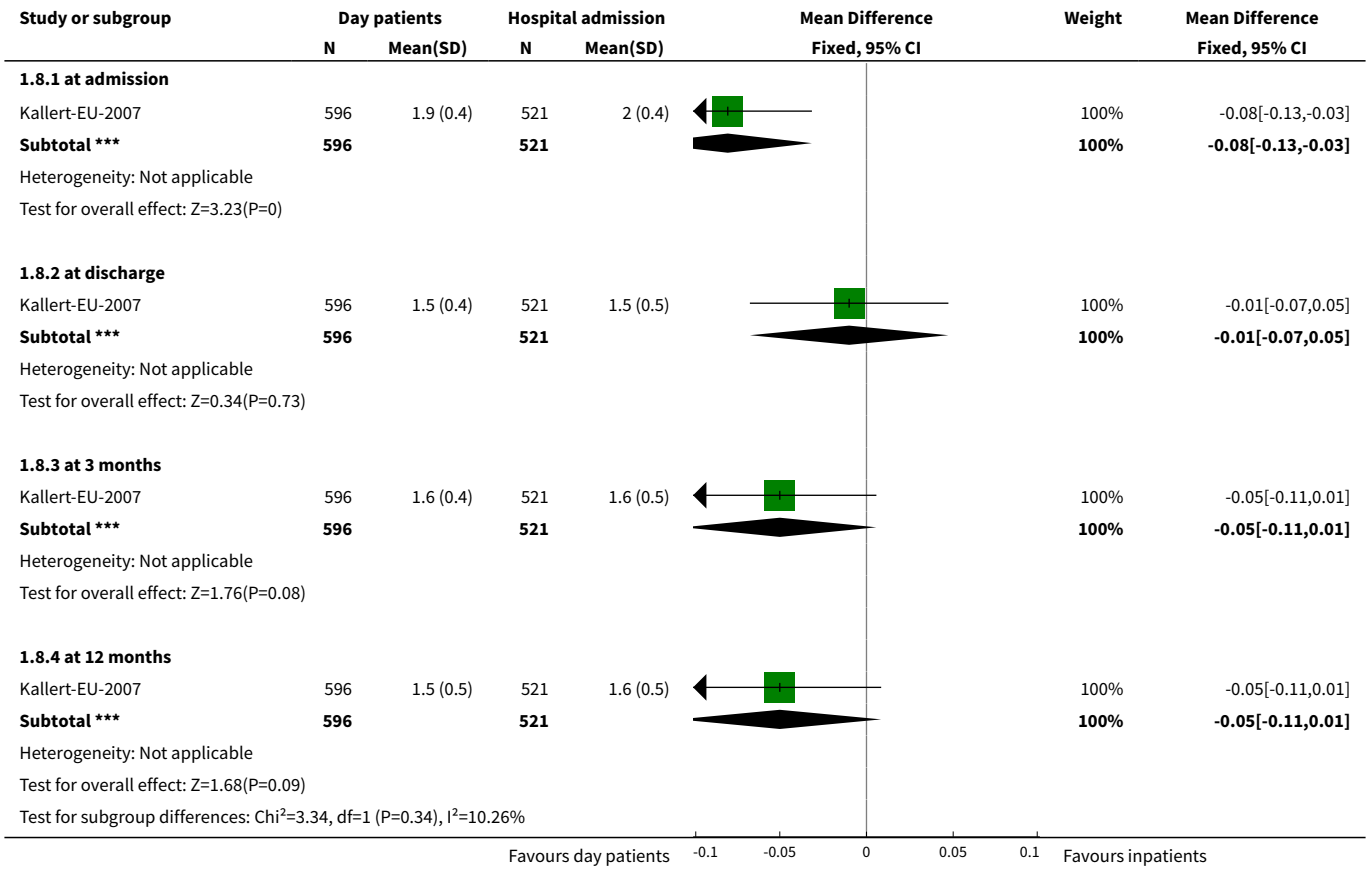


Favours day patients — Favours inpatients

Analysis 1.7. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 7 Extent of hospital care: 5. readmitted to in/day patient care after discharge.



Analysis 1.8. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 8 Mental state: average endpoint score (BPRS, high = poor).



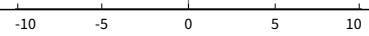
Analysis 1.9. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 9 Social functioning: average overall role score (GSDS-II, high = poor).

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
1.9.1 at admission							
Kallert-EU-2007	596	1.1 (0.6)	521	1.3 (0.6)		100%	-0.13[-0.2,-0.06]
Subtotal ***	**596**		**521**			**100%**	**-0.13[-0.2,-0.06]**
Heterogeneity: Not applicable							
Test for overall effect: Z=3.46(P=0)							
1.9.2 at discharge							
Kallert-EU-2007	596	0.9 (0.8)	521	1.2 (1.4)		100%	-0.34[-0.48,-0.2]
Subtotal ***	**596**		**521**			**100%**	**-0.34[-0.48,-0.2]**
Heterogeneity: Not applicable							
Test for overall effect: Z=4.9(P<0.0001)							
1.9.3 at 3 months							
Kallert-EU-2007	596	0.8 (0.7)	521	0.9 (0.8)		100%	-0.1[-0.19,-0.01]
Subtotal ***	**596**		**521**			**100%**	**-0.1[-0.19,-0.01]**
Heterogeneity: Not applicable							
Test for overall effect: Z=2.27(P=0.02)							
1.9.4 at 12 months							
Kallert-EU-2007	596	0.8 (0.7)	521	0.9 (0.8)		100%	-0.11[-0.19,-0.03]
Subtotal ***	**596**		**521**			**100%**	**-0.11[-0.19,-0.03]**
Heterogeneity: Not applicable							
Test for overall effect: Z=2.54(P=0.01)							
Test for subgroup differences: Chi^2=9.69, df=1 (P=0.02), I^2=69.04%							

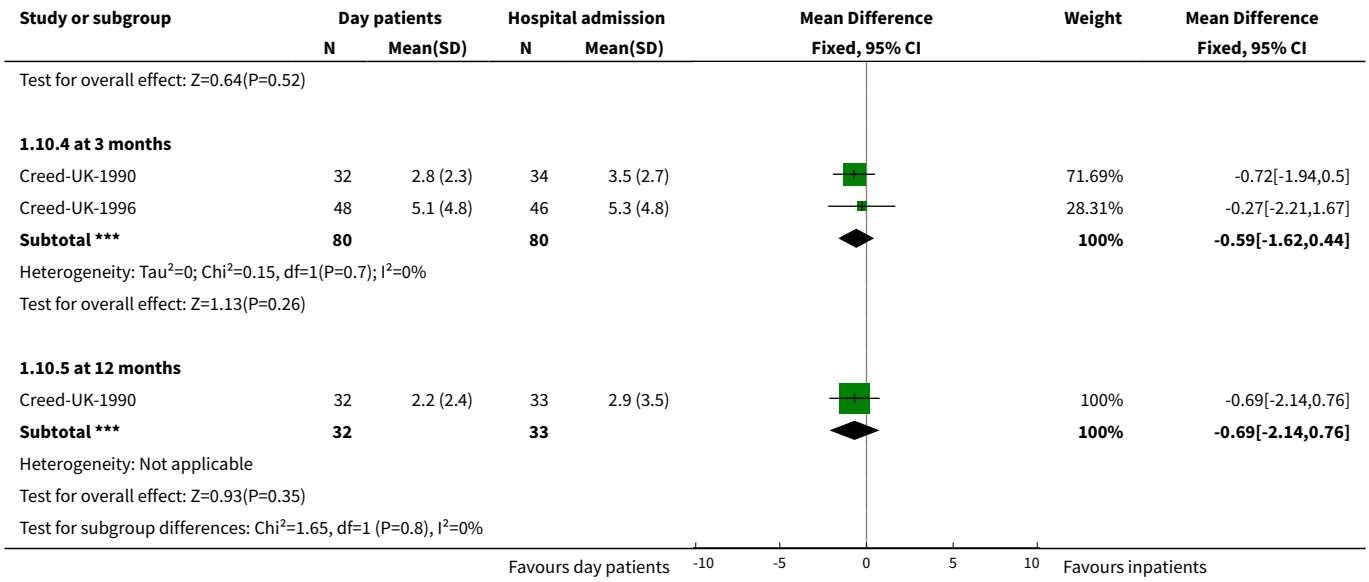
Favours day patients — Favours inpatients

Analysis 1.10. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 10 Burden: average carers' score (SBAS, high = poor).

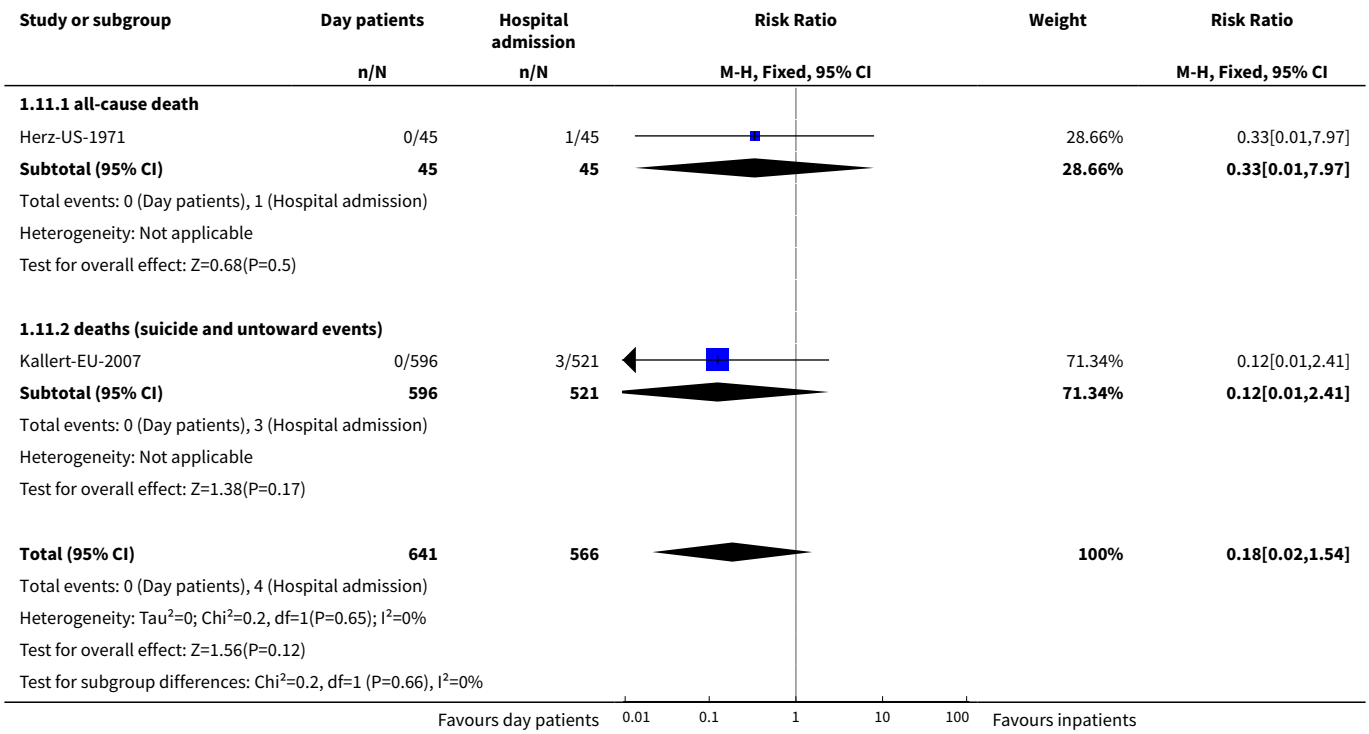
Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
1.10.1 at 14 days							
Creed-UK-1996	41	9.3 (7.2)	44	9.1 (6.1)		100%	0.27[-2.58,3.12]
Subtotal ***	**41**		**44**			**100%**	**0.27[-2.58,3.12]**
Heterogeneity: Not applicable							
Test for overall effect: Z=0.19(P=0.85)							
1.10.2 at 1 month							
Creed-UK-1996	46	7.7 (7)	49	7.8 (5.2)		100%	-0.04[-2.54,2.46]
Subtotal ***	**46**		**49**			**100%**	**-0.04[-2.54,2.46]**
Heterogeneity: Not applicable							
Test for overall effect: Z=0.03(P=0.98)							
1.10.3 at 2 months							
Creed-UK-1996	50	6.2 (5.5)	45	5.5 (4.4)		100%	0.65[-1.33,2.63]
Subtotal ***	**50**		**45**			**100%**	**0.65[-1.33,2.63]**
Heterogeneity: Not applicable							



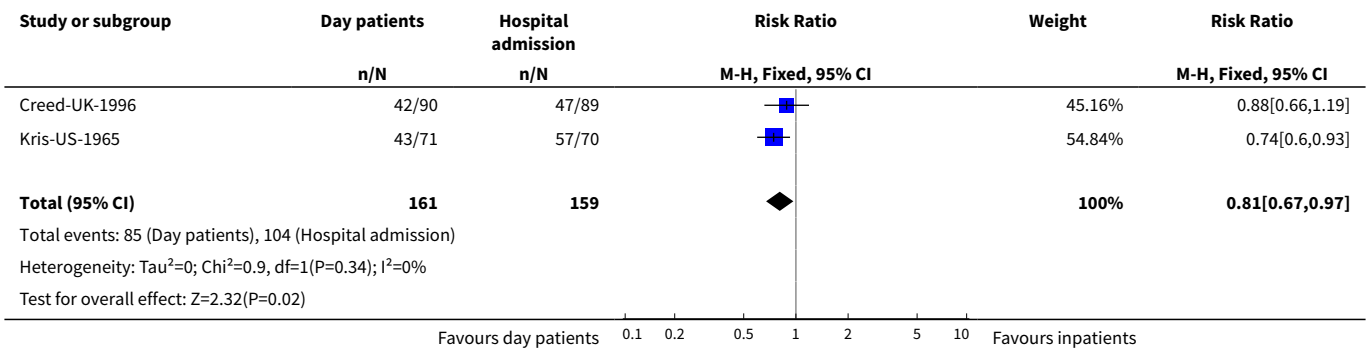
Favours day patients — Favours inpatients



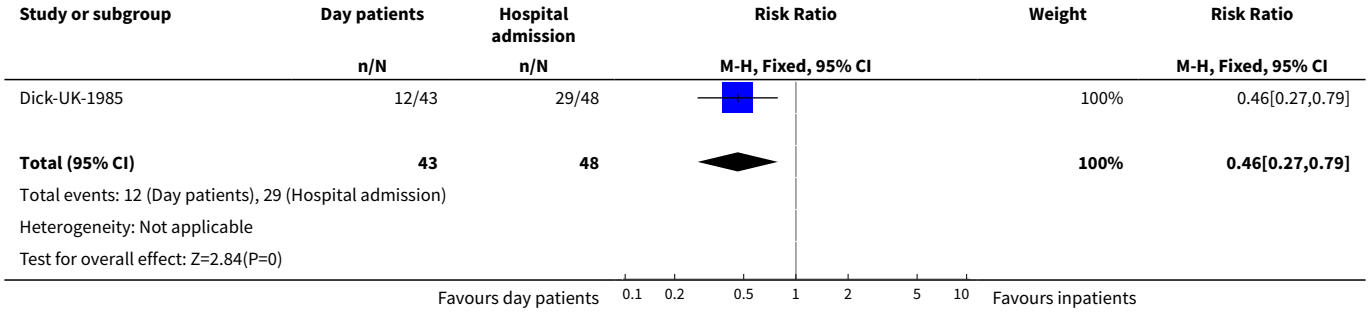
Analysis 1.11. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 11 Death (all causes).



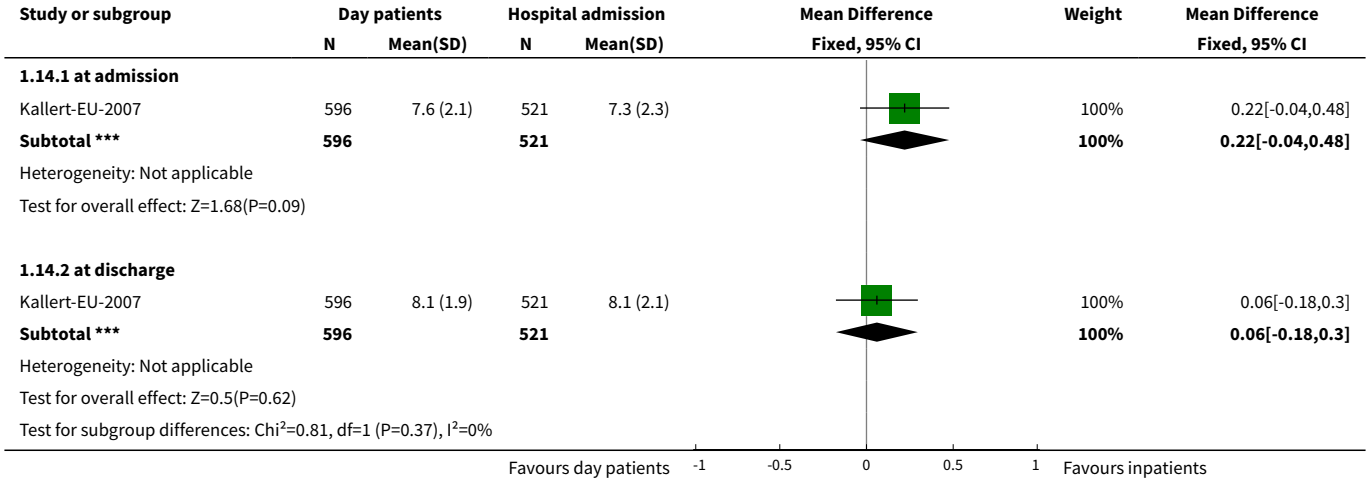
Analysis 1.12. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 12 Unemployed (at end of study).



Analysis 1.13. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 13 Satisfaction with care: 1. not satisfied with care received.



Analysis 1.14. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 14 Satisfaction with care: 2. average overall score (CAT, low = poor).



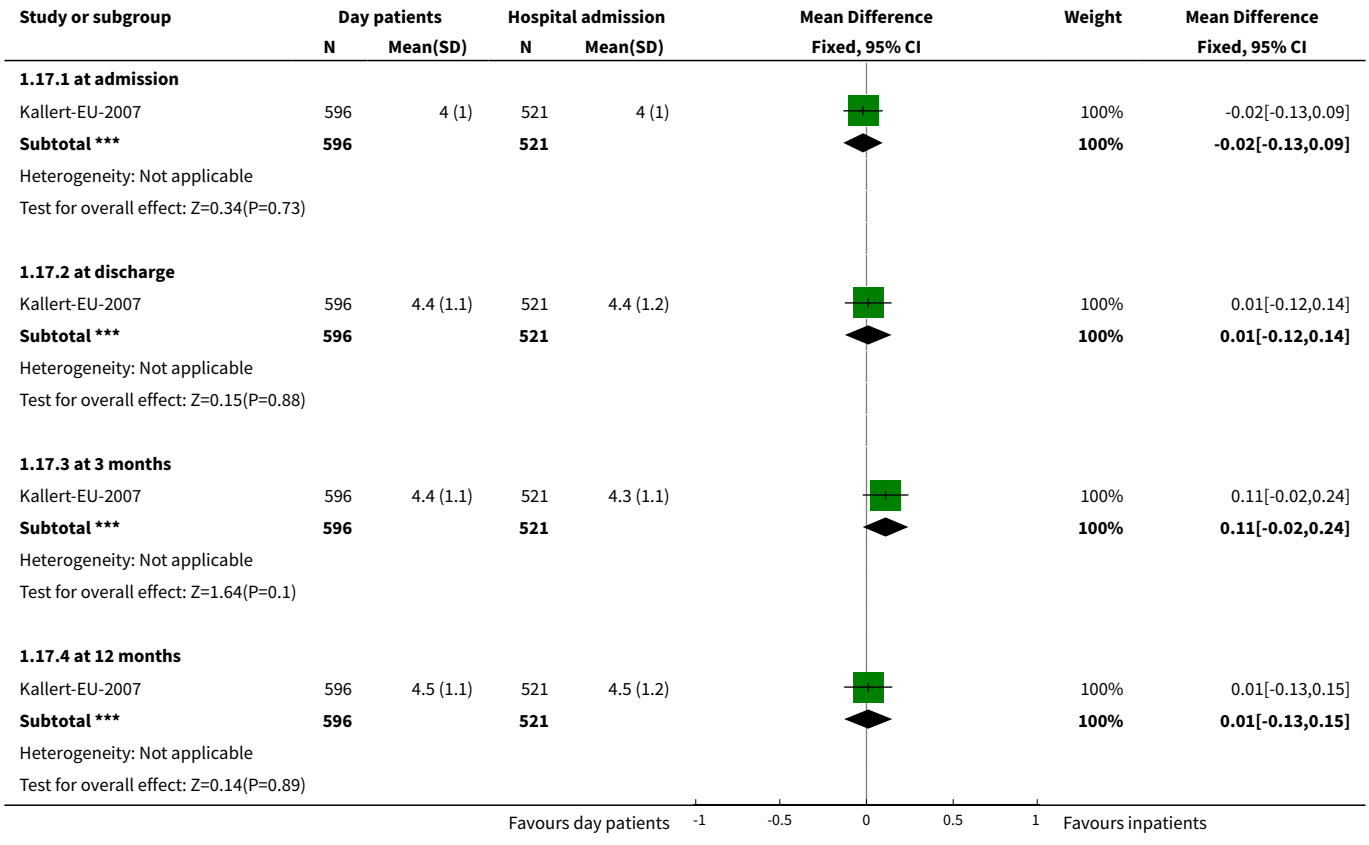
Analysis 1.15. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 15 Costs of care: 1. raw data.

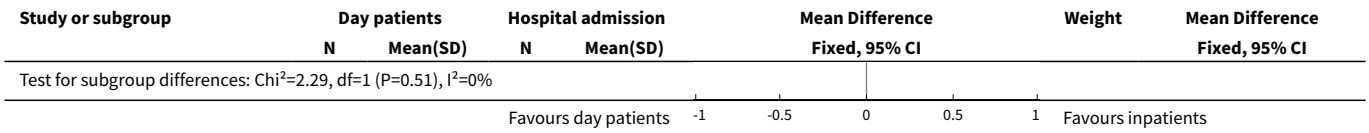
	Costs of care: 1. raw data							
Study	Index Ad. (DP)	Index Ad. (IP)	Hosp. Care (DP)	Hosp. Care (IP)	All Psy Care (DP)	All Psy Care (IP)	Total cost (DP)	Total cost (IP)
Creed-UK-1990	Not known	Not known	£4847 (3310-6384)	£6396 (4277-8515)	Not known	Not known	Not known	Not known
Creed-UK-1996	Not known	Not known	£4101 (2852-5351)	£6809 (5388-8231)	£4653 (3339-5966)	£7379 (5886-8872)	£5695 (2483-8907)	£7487 (5339-9636)
Dick-UK-1985	£307.3	£610.0	Not known	Not known	Not known	Not known	Not known	Not known
Sledge-US-1996	$13239 (9189-17288)	$19903 (15906-23899)	$24376 (18567-30186)	$30747 (24904-36590)	$26819 (20933-32705)	$33916 (27940-39893)	Not known	Not known

Analysis 1.16. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 16 Costs of care: 2. percent differences in costs.

	Costs of care: 2. percent differences in costs				
Study	Index Admission	Hospital care	All psychiatric care	All costs care	Notes
Creed-UK-1990	-49.6% (no test)	Not known	Not known	Not known	
Creed-UK-1996	Not known	-24.2% (p=0.675)	Not known	Not known	
Dick-UK-1985	Not known	-39.8% (p<0.001)	-36.9% (p<0.001)	-23.9% (p=0.014)	
Sledge-US-1996	-33.5% (p<0.001)	-20.7% (p=0.012)	-20.9% (p=0.009)	Not known	- indicates DH is cheaper Mann Whitney Tests

Analysis 1.17. Comparison 1 Day patient verus inpatient care for Type 1 studies, Outcome 17 Quality of life: average overall role score (MANSA, low = poor).





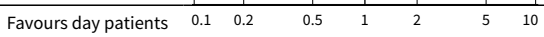
Comparison 2. Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised)

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Feasibility and engagement: lost to follow-up (at 2 years)	1	160	Risk Ratio (M-H, Fixed, 95% CI)	0.69 [0.48, 0.99]
2 Extent of hospital care: 1. duration of all hospital care (days/month, IPD - "nights in" & "nights out")	1	160	Mean Difference (IV, Fixed, 95% CI)	1.10 [-1.58, 3.78]
3 Extent of hospital care: 2. readmitted to in/day patient care after discharge	1	160	Risk Ratio (M-H, Fixed, 95% CI)	0.93 [0.64, 1.35]
4 Mental state: average endpoint score (PSE 9, high = poor, IPD)	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
4.1 at baseline	1	114	Mean Difference (IV, Fixed, 95% CI)	0.54 [-4.33, 5.41]
4.2 at 12 months	1	81	Mean Difference (IV, Fixed, 95% CI)	1.87 [-2.89, 6.63]
4.3 at 24 months	1	85	Mean Difference (IV, Fixed, 95% CI)	2.19 [-1.00, 7.38]
5 Social functioning: average overall role score (Groningen Scale, IPD)	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
5.1 at baseline	1	106	Mean Difference (IV, Fixed, 95% CI)	-0.11 [-0.45, 0.23]
5.2 at 12 months	1	95	Mean Difference (IV, Fixed, 95% CI)	-0.25 [-0.62, 0.12]
5.3 at 24 months	1	95	Mean Difference (IV, Fixed, 95% CI)	-0.19 [-0.58, 0.20]
6 Death (all causes)	1		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
6.1 deaths (suicide and untoward events)	1	160	Risk Ratio (M-H, Fixed, 95% CI)	2.79 [0.14, 57.10]
6.2 deaths (other causes)	1	160	Risk Ratio (M-H, Fixed, 95% CI)	0.37 [0.06, 2.14]
6.3 deaths all causes	1	160	Risk Ratio (M-H, Fixed, 95% CI)	0.74 [0.17, 3.18]
7 Unemployed (at end of study)	1	160	Risk Ratio (M-H, Fixed, 95% CI)	0.95 [0.87, 1.04]
8 Costs of care: 1. raw data			Other data	No numeric data

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
9 Costs of care: 2. percent differences in costs			Other data	No numeric data

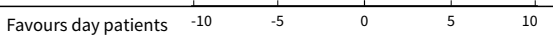
Analysis 2.1. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 1 Feasibility and engagement: lost to follow-up (at 2 years).

Study or subgroup	Day patients n/N	Hospital admission n/N	Risk Ratio M-H, Fixed, 95% CI	Weight	Risk Ratio M-H, Fixed, 95% CI
Wiersma-NL-1989	36/103	29/57		100%	0.69[0.48,0.99]
Total (95% CI)	**103**	**57**		**100%**	**0.69[0.48,0.99]**
Total events: 36 (Day patients), 29 (Hospital admission)					
Heterogeneity: Not applicable					
Test for overall effect: Z=2.01(P=0.04)					



Analysis 2.2. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 2 Extent of hospital care: 1. duration of all hospital care (days/month, IPD - "nights in" & "nights out").

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
Wiersma-NL-1989	103	7.5 (8.4)	57	6.4 (8.2)		100%	1.1[-1.58,3.78]
Total ***	**103**		**57**			**100%**	**1.1[-1.58,3.78]**
Heterogeneity: Not applicable							
Test for overall effect: Z=0.8(P=0.42)							



Analysis 2.3. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 3 Extent of hospital care: 2. readmitted to in/day patient care after discharge.

Study or subgroup	Day patients n/N	Hospital admission n/N	Risk Ratio M-H, Fixed, 95% CI	Weight	Risk Ratio M-H, Fixed, 95% CI
Wiersma-NL-1989	42/103	25/57		100%	0.93[0.64,1.35]
Total (95% CI)	**103**	**57**		**100%**	**0.93[0.64,1.35]**
Total events: 42 (Day patients), 25 (Hospital admission)					
Heterogeneity: Not applicable					
Test for overall effect: Z=0.38(P=0.7)					

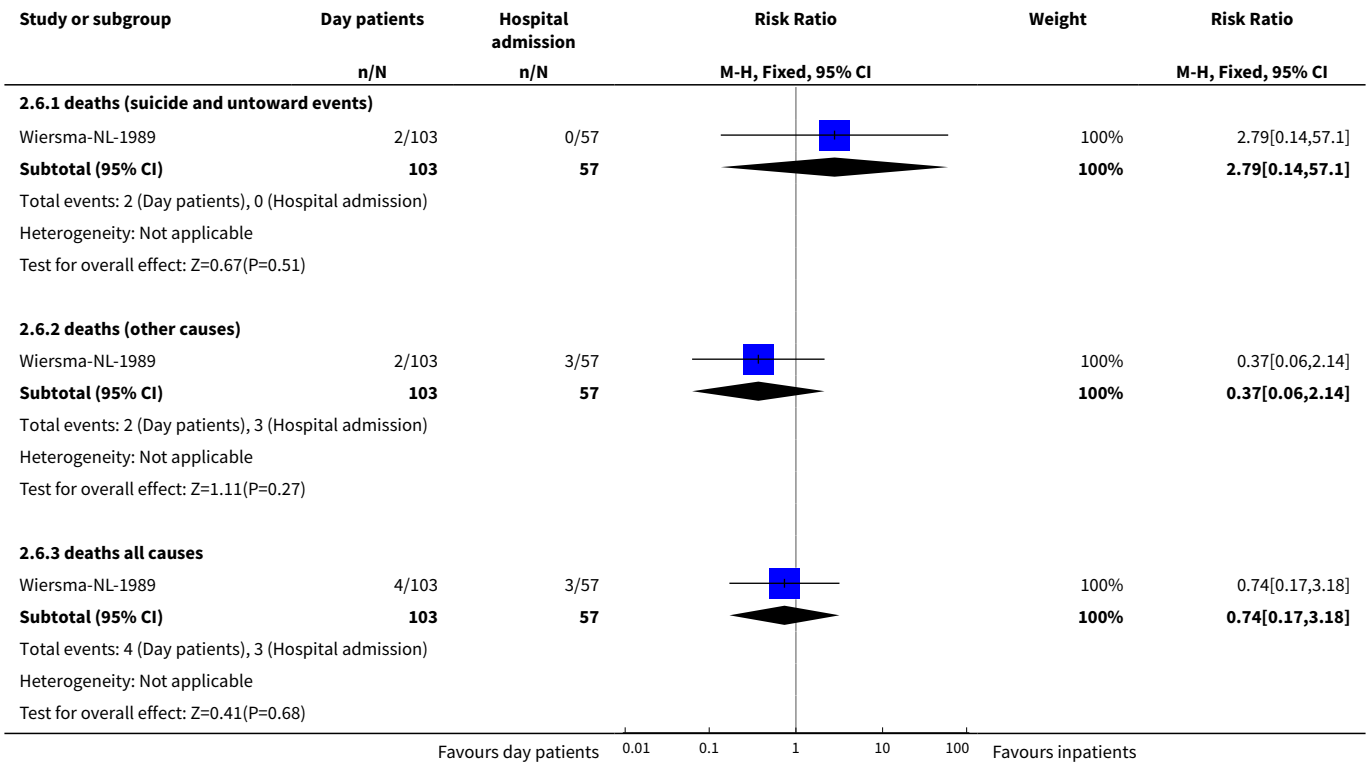
Analysis 2.4. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 4 Mental state: average endpoint score (PSE 9, high = poor, IPD).

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
2.4.1 at baseline							
Wiersma-NL-1989	77	21.3 (12.5)	37	20.8 (12.4)		100%	0.54[-4.33,5.41]
Subtotal ***	**77**		**37**			**100%**	**0.54[-4.33,5.41]**
Heterogeneity: Not applicable							
Test for overall effect: Z=0.22(P=0.83)							
2.4.2 at 12 months							
Wiersma-NL-1989	55	12.5 (12.9)	26	10.6 (8.6)		100%	1.87[-2.89,6.63]
Subtotal ***	**55**		**26**			**100%**	**1.87[-2.89,6.63]**
Heterogeneity: Tau^2=0; Chi^2=0, df=0(P<0.0001); I^2=100%							
Test for overall effect: Z=0.77(P=0.44)							
2.4.3 at 24 months							
Wiersma-NL-1989	60	10.6 (11.5)	25	8.4 (11)		100%	2.19[-3,7.38]
Subtotal ***	**60**		**25**			**100%**	**2.19[-3,7.38]**
Heterogeneity: Not applicable							
Test for overall effect: Z=0.83(P=0.41)							
Test for subgroup differences: Chi^2=0.24, df=1 (P=0.89), I^2=0%							

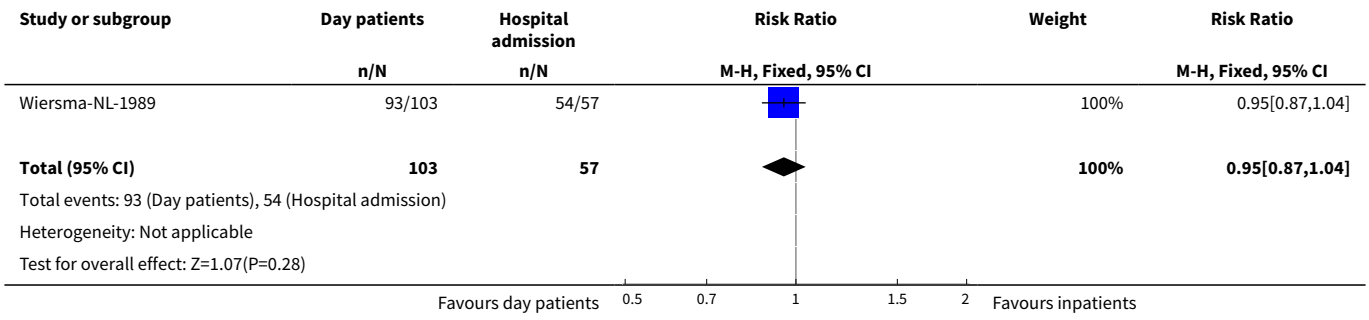
Analysis 2.5. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 5 Social functioning: average overall role score (Groningen Scale, IPD).

Study or subgroup	Day patients N	Day patients Mean(SD)	Hospital admission N	Hospital admission Mean(SD)	Mean Difference Fixed, 95% CI	Weight	Mean Difference Fixed, 95% CI
2.5.1 at baseline							
Wiersma-NL-1989	74	2.4 (0.8)	32	2.5 (0.8)		100%	-0.11[-0.45,0.23]
Subtotal ***	**74**		**32**			**100%**	**-0.11[-0.45,0.23]**
Heterogeneity: Not applicable							
Test for overall effect: Z=0.63(P=0.53)							
2.5.2 at 12 months							
Wiersma-NL-1989	67	2.2 (0.9)	28	2.5 (0.8)		100%	-0.25[-0.62,0.12]
Subtotal ***	**67**		**28**			**100%**	**-0.25[-0.62,0.12]**
Heterogeneity: Not applicable							
Test for overall effect: Z=1.31(P=0.19)							
2.5.3 at 24 months							
Wiersma-NL-1989	67	2.1 (0.9)	28	2.3 (0.9)		100%	-0.19[-0.58,0.2]
Subtotal ***	**67**		**28**			**100%**	**-0.19[-0.58,0.2]**
Heterogeneity: Not applicable							
Test for overall effect: Z=0.95(P=0.34)							
Test for subgroup differences: Chi^2=0.3, df=1 (P=0.86), I^2=0%							

Analysis 2.6. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 6 Death (all causes).



Analysis 2.7. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 7 Unemployed (at end of study).



Analysis 2.8. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 8 Costs of care: 1. raw data.

	Costs of care: 1. raw data			
Study	Hosp. care (DP)	Hosp. care (IP)	All Psy Care (DP)	All Psy Care (IP)
Wiersma-NL-1989	Dfl 43928 (33535-54319)	Dfl 35990 (23375-48604)	Dfl 48377 (38005-58748)	Dfl 38252 (25684-50821)

Analysis 2.9. Comparison 2 Day patient versus inpatient care for Type 2 trials (all presenting for admission were randomised), Outcome 9 Costs of care: 2. percent differences in costs.

	Costs of care: 2. percent differences in costs		
Study	**Hospital care**	**All psychiatric care**	**Notes**
Wiersma-NL-1989	+22.0% (p=0.175)	+26.4% (p=0.057)	+ indicates DH is more expensive

ADDITIONAL TABLES

Table 1. Feasibility and engagement: 1. proportion suitable for day hospital (Type 1)

Study	Eligible (pess)	Eligible (opt)	Randomised	Rand day hosp	Rand & engaged	% feasible (opt)	% feasible (pess)
Kris-US-65	?	?	not applicable	not applicable	?	?	? (see text)
Herz-US-71	424	310	90	45	35	29.0	16.5
Dick-UK-85	334	203	75	43	37	36.9	19.3
Creed-UK-90	185	175	102	51	35	58.3	37.8
Schene-NL-93	534	534	199	?	?	37.3	? (see text)
Creed-UK-96	?	?	not applicable	not applicable	?	?	? (see text)
Sledge-US-96	791	546	197	93	93	36.1	24.9
Overall type 1	2268	1768	663	232	200	37.5 (95% CI 35.2-39.8)	23.2 (95% CI 21.2-25.2)

Table 2. Type 1 trials: data schedule for individual patient data

Trial	Mental State	Social Functioning
Creed-UK-1990	0, 3 & 12 months	0, 3 & 12 months
Creed-UK-1996	0, 6 & 12 months	0, 0.5, 1, 2, 3, 6 & 12 months
Sledge-US-1996	2, 5 & 10 months	0, 2, 5 & 10 months

Table 3. Type 1 trials: summary of covariates used in the individual patient analysis

Co-variate	Creed 1990 (IP)	Creed 1990 (DP)	Creed 1996 (IP)	Creed 1996 (DP)	Sledge 1996 (IP)	Sledge 1996 (DP)
N randomised	51	51	93	94	104	93
N included in analysis	47	40	84	84	98	91
Males (%)	26 (55)	23 (58)	45 (54)	46 (55)	56 (57)	42 (46)
Females (%)	21 (45)	17 (42)	39 (46)	38 (45)	42 (43)	49 (54)
< 24 yrs	2 (4)	4 (10)	14 (17)	14 (17)	15 (15)	15 (16)
25-34	20 (42)	10 (25)	23 (27)	24 (28)	43 (44)	42 (46)
35-44	7 (15)	6 (15)	25 (30)	17 (20)	23 (23)	20 (22)
45-54	8 (17)	7 (18)	13 (15)	9 (11)	10 (10)	11 (12)
> 55	10 (21)	13 (32)	9 (11)	20 (24)	7 (7)	3 (3)
Bipolar or scz	18 (38)	14 (35)	40 (48)	31 (40)	56 (57)	46 (50)
Other diagnosis	29 (62)	26 (65)	44 (52)	53 (60)	42 (43)	45 (49)

Table 4. Mental state: model coefficients for standardised mental state scores

Parameters	Model Coeff. (SE)	95% CI	P value
FIXED EFFECTS			
Time intervention interaction (months)	-0.007 (0.0022)	-0.011 to -0.002	0.002
Time (months)	-0.073 (0.0067)	-0.086 to -0.059	
Gender (0 = female, 1 = male)	0.018 (0.0642)	-0.110 to 0.147	0.777
Diagnosis (0 = other, 1 = scz or bpd)	0.054 (0.0648)	-0.076 to 0.184	0.406
Age	0.019 (0.1124)	-0.206 to 0.244	0.862

Table 4. Mental state: model coefficients for standardised mental state scores *(Continued)*

Study 2 (Creed-UK-1996)	-0.046 (0.0899)	-0.225 to 0.134	
Study 3 (Sledge-US-1996)	0.084 (0.0948)	-0.106 to 0.273	0.189
Intervention group	0.144 (0.0671)	0.009 to 0.278	0.032
Constant	0.229 (0.1303)	-0.026 to 0.485	
RANDOM EFFECTS (patient level)			
Constant (intercept)	0.211 (0.0324)	n/a	
Constant*time (weeks)	0.001 (0.0007)	n/a	
Time gradient (weeks)	0.00008 (0.00003)	n/a	
RANDOM EFFECTS (time level)			
Constant (error)	0.508 (0.0225)	n/a	

Table 5. Social functioning: model coefficients for standardised social functioning score

Parameters	Model Coeff (SE)	95% CI	P value
FIXED EFFECTS			
Time-intervention interaction (months)	-0.001 (0.0121)	-0.025 to 0.023	0.941
Time (months)	-0.052 (0.0087)	-0.069 to -0.034	
Gender	0.404 (0.0862)	0.231 to 0.576	0.001
Diagnosis	0.087 (0.0854)	-0.084 to 0.257	0.310
Age	-0.100 (0.0356)	-0.171 to -0.028	0.005
Study 2 (Creed-UK-1996)	-0.010 (0.1158)	-0.241 to 0.222	
Study 3 (Sledge-US-1996)	-0.010 (0.1094)	-0.229 to 0.209	0.995
Intervention group	-0.041 (0.1098)	-0.261 to 0.179	0.708
Constant	0.344 (0.1698)	0.011 to 0.677	
RANDOM EFFECTS			
Patient level (constant - intercept)	0.313 (0.0440)	n/a	
Time level (constant - error)	0.565 (0.0343)	n/a	

Table 6. Feasibility and engagement: 2. proportion suitable for day hospital (Type 2)

Study	Eligible	Randomised	Rand day hosp	Mainly in DH	% feasible
Wiersma-NL-89	160	160	103	19	18.4
Zwerling-US-64	278	189	189	74	39.1

APPENDICES

Appendix 1. Search methods for identification of studies on the previous version of the review

a. Electronic searches

The search began by deriving a list of search terms from reading overviews of the field and consulting experts in day hospital care. The reference databases listed below were searched using Ovid Biomed.

1. CINAHL (January 1982 - December 2000) was searched using the Cochrane Schizophrenia Group's search strategy for randomised controlled trials combined with the phrase: [((DAY adj2 HOSP*) or (DAY adj2 CARE) or (DAY adj2 TREATMENT*) or (DAY adj2 CENT*) or (DAY adj2 UNIT*) or (PARTIAL adj2 HOSP*) or (DISPENSARY)) AND MENTAL DISORDERS].

2. The Cochrane Library (Issue 4, 2000) was searched using the phrases: [((DAY near HOSP*) or (DAY near CARE) or (DAY near TREATMENT*) or (DAY near CENT*) or (DAY near UNIT*) or (PARTIAL near HOSP*) or (DISPENSARY)) AND MENTAL DISORDERS exploded].

3. EMBASE (January 1980 - December 2000) was searched using the Cochrane Schizophrenia Group's search strategy for randomised controlled trials combined with the phrase: [((DAY adj2 HOSP*) or (DAY adj2 CARE) or (DAY adj2 TREATMENT*) or (DAY adj2 CENT*) or (DAY adj2 UNIT*) or (PARTIAL adj2 HOSP*) or (DISPENSARY)) AND MENTAL DISORDERS].

4. MEDLINE (January 1966 - December 2000) was searched using the Cochrane Schizophrenia Group's search strategy for randomised controlled trials combined with the phrase: [((DAY adj2 HOSP*) or (DAY adj2 CARE) or (DAY adj2 TREATMENT*) or (DAY adj2 CENT*) or (DAY adj2 UNIT*) or (PARTIAL adj2 HOSP*) or (DISPENSARY)) AND MENTAL DISORDERS/All subheadings exploded].

5. PsycLIT (January 1967 - December 2000) was searched using the Cochrane Schizophrenia Group's search strategy for randomised controlled trials combined with the phrase: [((DAY adj2 HOSP*) or (DAY adj2 CARE) or (DAY adj2 TREATMENT*) or (DAY adj2 CENT*) or (DAY adj2 UNIT*) or (PARTIAL adj2 HOSP*) or (DISPENSARY)) AND MENTAL DISORDERS].

b. Searching other resources

1. Reference searching
The sensitivity of the search strategy was examined by comparing the results of the search with the reference lists of the identified reviews and trials, but no new trials were identified.

2. Personal contact
Researchers in the field were approached to identify unpublished studies.

Appendix 2. Modifications to original protocol

1. After writing the initial protocol it became obvious that it would be difficult to synthesis summary data from the included trials because of the range and complexity of the outcome variables that had been used. For example, one key outcome, use of hospital care, had been reported in terms of days in inpatient care, duration of day patient care, adjusted duration of day care (discounting weekends and days off), duration of index admission, nights out of hospital, actual attendances at day care, readmission to day care, readmission to inpatient care and so on. The result was that whilst most acute day hospital trials reported similar outcomes, these outcomes were rarely in the same format and hence could not be combined across trials. The picture was further complicated because many of the outcome variables were skewed, and tended to be presented in forms (such as medians) which cannot be readily synthesised in a meta analysis. It was therefore considered essential to obtain individual patient data from included trials so that the relevant outcomes could be presented in a common format.

2. The original protocol proposed to look at a number of different ways of using day hospitals, in addition to using them as an alternative to admission. This was too large a project to be contained in a single review, so alternative uses of day hospitals are covered in a separate review (Marshall 2001).

3. The original protocol did not propose to look at feasibility of day hospital treatment. On reading the original papers and reviews it became clear that this was an important question that should be addressed by the review. Feasibility was therefore added to the list of outcomes.

WHAT'S NEW

Date	Event	Description
17 February 2011	New search has been performed	New search carried out June 2010, results incorporated into review. Protocol: methods section updated.
17 February 2011	New citation required but conclusions have not changed	One new study added to included studies, three studies added to excluded studies, no substantive change to results.

HISTORY

Protocol first published: Issue 1, 1999
Review first published: Issue 1, 2003

Date	Event	Description
22 October 2008	Amended	Converted to new review format.

CONTRIBUTIONS OF AUTHORS

Max Marshall - conceived the review and obtained funding, designed the review, co-ordinated the review and collected the data, developed and screened the results of the search strategy, appraised papers and extracted data, compiled the individual patient data and cross-checked it against the trial reports, carried out the analyses of individual patient data other than mental state and social functioning, interpreted the data and wrote the final report.

Ruth Crowther - co-ordinated the review and collected the data, updated the searches, cross-checked the data extraction, managed the data for the review and entered data on RevMan, compiled the individual patient data and cross-checked it against the trial reports, and advised on the final report.

William Sledge - prepared and provided individual patient data, provided additional information about their trials as requested, advised on interpretation of data, and the final report.

John Rathbone - extracted data for 2011 update.

Karla Soares-Weiser - extracted data and rewrite of text 2011 update.

DECLARATIONS OF INTEREST

Francis Creed, William Sledge, Herman Kluiter and Durk Wiersma have carried out trials of acute day hospital treatment.

SOURCES OF SUPPORT

Internal sources

- Guild Community Healthcare Trust, UK.

External sources

- NHS Health Technology Assessment - grant no. 96/41/3, UK.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

The protocol has been updated to reflect new methodology used in Cochrane reviews, for example inclusion of Summary of findings for the main comparison.

INDEX TERMS

Medical Subject Headings (MeSH)

*Day Care, Medical; *Hospitalization; Acute Disease; Length of Stay; Mental Disorders [*therapy]; Psychotic Disorders [therapy]; Randomized Controlled Trials as Topic

MeSH check words

Humans